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THE

PHILADELPHIA JOURNAL

OF THE

MEDICAL AND PHYSICAL SCIENCES.

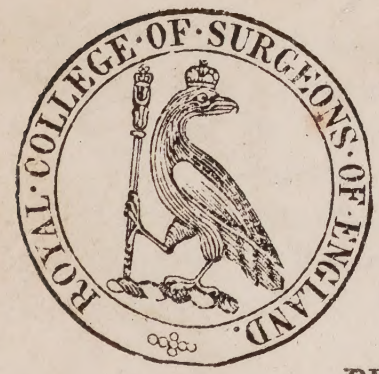
SUPPORTED BY AN ASSOCIATION OF PHYSICIANS,

AND

EDITED BY N. CHAPMAN, M. D.
PROFESSOR OF THE INSTITUTES AND PRACTICE OF PHYSIC AND CLINICAL PRACTICE, IN THE UNIVERSITY OF PENNSYLVANIA.

"In the four quarters of the globe, who reads an American book? or goes to an American play? or looks at an American picture or statue? *What does the world yet owe to American Physicians or Surgeons?*"

Edinburgh Review, No. LXV.



VOL. IV.

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24

Eastern District of Pennsylvania, to wit:

BE IT REMEMBERED, that on the twenty-seventh day of April, SEAL. in the forty-sixth year of the Independence of the United States of America, A. D. 1822, H. C. Carey & I. Lea, of the said District, have deposited in this office the title of a Book, the right whereof they claim as proprietors, in the words following, to wit:

"The Philadelphia Journal of the Medical and Physical Sciences. Supported by an Association of Physicians, and edited by N. Chapman, M. D. Professor of the Institutes and Practice of Physic and Clinical Practice in the University of Pennsylvania.

"In the four quarters of the globe, who reads an American book? or goes to an American play? or looks at an American picture or statue? *What does the world yet owe to American Physicians or Surgeons?*"

Edinburgh Review, No. LXV.

In conformity to the Act of the Congress of the United States, entitled, "An Act for the encouragement of Learning, by securing the Copies of Maps, Charts, and Books, to the Authors and Proprietors of such Copies, during the times therein mentioned."—And also to the Act, entitled, "an Act supplementary to an Act, entitled, "an Act for the encouragement of Learning, by securing the Copies of Maps, Charts, and Books, to the Authors and Proprietors of such Copies during the times therein mentioned," and extending the benefits thereof to the Arts of designing, engraving, and etching historical and other Prints."

D. CALDWELL,
Clerk of the Eastern District of Pennsylvania.

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TO READERS AND CORRESPONDENTS.

1. We have received from a friend, to whom we tender our acknowledgments for the favour, the bills of mortality of most of our large cities, which shall appear in our next number. Estimating indeed very highly the importance of such intelligence, we mean hereafter regularly to appropriate a part of this Journal to the subject. We shall also very gladly insert, accurate tables of meteorological observations, made in any section of our country, of which we are now in possession of a considerable number, that shall be published without delay.

2. The 7th and last volume of the Transactions of the Royal Medical Society of Copenhagen, has been transmitted to us, with a testimonial of the kindness of that learned body to ourselves, of which we are very sensible. The volume is altogether an interesting one, and we shall take an early opportunity of laying before our readers some account of its contents.

3. We wish it to be distinctly understood, that we neither have, nor will receive, any pecuniary compensation as Editor of this Journal. The motives which led us to engage in the enterprise, are announced in our prospectus, and which will be found liberal, and wholly disinterested. To this subject attention is now called, with a request, that communications for the work, and all matters of correspondence relative to it, may be addressed to the publishers, Messrs. H. C. Carey and I. Lea, Booksellers, Philadelphia.

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THE

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ART. I. *An account of the Yellow Fever in the River Oronoko, in August,* 1819. By M. MORGAN, M. D. U. S. Navy.

AFTER the many excellent treatises on tropical diseases, from such men as Ferguson and Johnson, and the valuable works on yellow fever by Dr. Rush and others, an entire enumeration of symptoms, or a minute history of treatment so generally known to practitioners, would appear tedious and unnecessary. But as too much cannot be said upon the subject, provided we relate facts, since the aspects of endemic and epidemical diseases are so frequently modified by unknown causes and general doctrines in therapeutics difficult of correct application, it is yet the duty of every physician who sees yellow fever, to lay his experience before the medical world. Nor should he be deterred from it by flattering himself, that a coincidence of opinion has taken place on any of the several important points which have heretofore furnished matter of controversy. In all our large cities there are physicians (and some of them men whose opinions are not founded on the seductions of delusive theory, but by a scrupulous collection of facts) who still think that the excretions of the infected body are dangerous

in reproducing the disease, and that only a tropical climate can commence its generation. The popular mind being also too strongly inclined to this belief, an accumulation of knowledge on the subject must continue to be a desideratum.

But it is in a practical point of view, that it presents the highest claims to our attention. The fevers of the tropics in their first attack of a healthy person from Europe or North America, are rapid in their progress, and generally, in the early stage, attended with inordinate reaction. Although not a principle of universal application, yet few have had more confirmation, and there is reason to believe that these diseases too frequently find their victims from a timorous employment of the lancet and mercury, reliance being placed on mild cathartics, diluents and diaphoretics.

The United States corvette John Adams and schooner Nonsuch, sailed from the Chesapeake on the 12th of June, 1819, touched at Barbadoes on the 5th of July, and proceeded to the mouth of the Oronoko. On the 8th we discovered land, and continued on the coast till the 15th, when the Nonsuch took a pilot and proceeded up the river, and the John Adams went to Port Spain (Trinidad) to await our return.

During the week we spent on the coast, the weather was very unpleasant. Sultry mornings were succeeded by heavy and frequent showers, and these followed by hot sunshine. The crews continued healthy. There was no sickness among the pilots who lived on a small island near the mouth of the river, nor among the Indians who came to us from time to time in their canoes. The river, generally three or four hundred yards wide, was rising rapidly, and inundated a vast country adjacent to the ocean. There were a number of islands within the first fifty or sixty miles, its banks were every where covered with trees and thick underwood, with a luxuriant growth of vegetation, so that we could see only a few yards from the water. The weather was hot, the thermometer 80° to 90° Fahr.; some showers fell, but the sea-

breeze relieved us usually in the afternoon, and it became quite comfortable. No sickness prevailed in the few scattering villages along the river. The inhabitants seemed to avoid laborious occupation, and subsisted chiefly on bananas, plantains, sugar cane, and casada, but had also plenty of animal food. After the first hundred miles, the highlands begin to appear, the country is broken and irregular, the hills abound plentifully with stone, and are covered with low forest trees, and the vallies are rich pastures. We arrived at the place of destination on the 27th of July, all well.

Angostura is situated about three hundred miles up the river, on the south bank, and contains five or six thousand inhabitants. The site is high, the streets dry, and some of them well paved. The country around was but imperfectly cultivated, is broken with hills and ravines, and covered with a luxuriant growth of tropical vegetation. The latitude of the town is about eight degrees north, and differs very little from that mouth by which we entered in ascending the river. The soil is a loose earth mixed with pebbles, in some places alluvial, from the overflowings of the river; in others a surface of clay, which in dry weather bakes very hard. The waters were clear and pure.

The river is more than half a mile wide, extremely deep, muddy and rapid, and rises, towards September, to a height of thirty or forty feet perpendicular to its banks. Its course, opposite the town, is nearly from west to east.

During the day we felt the heat oppressive, the thermometer being from 80° to 96° Fahrenheit, but at night cool easterly breezes came on, and light covering became agreeable.

No acute case of disease occurred, while we were ascending the river. The yellow fever prevailed in the town when we arrived, and was very fatal to the English troops and their families, who had come to the country to assist the independents. The creoles and other inhabitants also died with it, though with them it was comparatively mild.

Our vessel was anchored about twenty paces from the

bank, and a strictly regulated police enjoined. The crew was as little exposed as possible to the sun, the men were kept temperate as to drink, and were interdicted from exposure to night air on shore. Fresh and salt provisions were daily alternated, and they had a sufficient quantity of wholesome vegetables. They were allowed to sleep on deck under an awning, or below, whichever might be most agreeable to them.

All our precautions however were of little avail. On the 1st of August, the cabin steward was taken violently, and his symptoms were so similar to those of the English patients I had seen on shore, that no doubt could exist, of his having the prevailing fever.

He had been often sent to market and on other errands, and was addicted to intemperance. The symptoms of the fever as they occurred with him and the subsequent cases were as follows.

They complained of lassitude and headach—the eyes were red, watery and languid—in some cases a slight sense of coldness was felt, and the features were for a short time shrunk: the skin was hot and dry, the tongue moist and covered with a light yellowish fur. Pains in the loins came on, and the patient was obliged to lie down: in a little time all the symptoms were in a great degree increased. The pain in the head and loins became intolerable—the pulse was full, strong and frequent—great restlessness came on, so that the patient could lie but a moment in the same position, and loud complaints were uttered. In some instances the patient was delirious in two or three hours. The bowels were obstinately constipated, and vascular action so vehement that the pulsations of the carotid and temporal arteries were strikingly visible. Some complained most of the head, the pain being immediately above the eyes: others were most distressed by the pain in the loins.

A general prejudice existing against venesection on shore, where it was practiced, I thought best to adopt it with such caution as to avoid the imputation of rashness, although it appeared to be strongly indicated in the early stage of every

case I had seen in the hospital which I visited. Accordingly I commenced the treatment of the first patient by evacuating the stomach and bowels with sulph. soda one ounce, emetic tart. one grain. Although the evacuations were copious, no considerable abatement of the symptoms had taken place, and in the evening I resolved to bleed him as it was obvious no time ought to be lost. A vein was opened, and when a considerable quantity had been taken, on being asked how he felt, he said he was much easier; and afterwards, while the blood was flowing, repeated, that he felt relief from the pain in his head and back. His pulse became slower and softer, and when he had lost about twenty ounces, he was quite easy and tranquil. He was sponged with vinegar and water, his head and hands constantly wetted with it, and again purged with calomel and jalap, each ten grains. The next morning his skin had become cool and moist, his pains had nearly left him, his pulse was slow and regular, and in four or five days he was able to walk about. Great gastric irritability however remained, and for some time during convalescence, scarcely any thing could be retained on his stomach. This was the most manageable case I had, and encouraged me to use the remedies with more confidence. On the second day of the first case, a man reported himself who had been assisting to cut grass on the north side of the river. The place where the grass grew was a little swampy, and not as yet reached by the rising of the river.

His attack was very severe: he told no one of his illness till he was no longer able to stand, and he died in thirty-six hours with black vomit. His nurse having fallen asleep, I found him about midnight walking delirious on deck—and, while putting him to bed, he vomited a large quantity of black matter, and died in a few minutes afterwards.

From this time three or four were taken daily until nearly half the officers and men were down with the disease, and it took all that were well to attend the sick.

There was scarcely from the first an interval between the

state of lassitude and that of high inflammatory excitement. The following comprises the treatment pursued.

In a few cases where any thing was done during the state of depression, the stomach and bowels were emptied by sulph. soda and emetic tartar, and venesection and the other remedies used afterwards. But in most cases, the patient first stript of all his clothes except his shirt, was laid in his bed on deck under a thick awning, and blood taken from him according to his constitution, and the violence of the symptoms. In some cases sixteen ounces were taken, and in others thirty or forty. A strong cathartic was given, mostly calomel and jalap, each one scruple. The hair was cut off as close as it could be with a pair of scissors, and the head being laid over a wide basin, cold water and vinegar mixed, was wrung on it from a large sponge, or poured from a tin for ten or fifteen minutes at a time, and this repeated at intervals. The body was sponged frequently with cold water, and the hands kept almost constantly immersed in it. This was found so agreeable to the patient as not to be dispensed with, and produced great mitigation of all the symptoms.

Sometimes the purging was commenced with sulph. soda, and emet. tart. aided by enemata, and followed immediately after operating, by calomel and jalap, each twelve grains: this was thought at first the quickest and most effectual way of emptying the bowels. But I afterwards gave calomel and jalap, each one scruple, directly after bleeding—which made the discharges more copious, less frequent, and relieved the patient more.

Enemata were of the greatest utility. The lower bowels were always loaded, and by discharging the contents of these, the operation of purgatives was much accelerated. Without them the largest doses of medicine would be delayed five or six hours.

In administering cathartics, the jalap was diminished after the first. Two cathartics were given in immediate succession, and often before the first had entirely ceased operating. The second, in some instances, brought away almost as much of the alvine contents, as the first.

Some were purged on the third day, but if the disease was not decisively arrested within the first twenty-four or thirty-six hours, the patient generally died.

The bleeding was rarely repeated more than once; but several of the cases had such a return of pain, as to render a second bleeding requisite. There was scarcely time for the employment of blisters: much advantage was derived from hot spirit of turpentine in one case, and I should be inclined in future to rub it over the epigastrium. The stomach during convalescence was always in a state of such weakness and irritability, that nothing would remain on it, however mild or cordial. In one case, where looseness of the bowels occurred, I did not purge enough, being intimidated by this symptom. One man, who had been under mercurial treatment for syphilis, had the fever: his case was a mild one.

The most fatal attacks were among the men who had been cutting grass; only one recovering of four who took the disease. It was exceedingly severe and mortal to men of what has been denominated the bilious temperament: persons of sandy complexion and sanguine predisposition, recovered more happily.

It is impossible to say where the disease commenced, or what function was first deranged or interrupted—though it appeared most probable, that the brain received the primary morbid impression, and communicated distraction and disorder to every part of the system.

The cold affusion I did not try, having, as I then thought, obtained more permanent advantages, by the gradual use of cold water frequently repeated.

Anodyne diaphoretics were very salutary, after the reduction of vascular excitement, and the disease was broken by other remedies.

Bitters were very usefully employed during convalescence, which was rather slow, and proved highly agreeable to the stomach.

Under this treatment, one fourth of my patients died, but I am far from thinking that a milder or more dilatory prac-

tice would have been attended with equal success. On the contrary, I do not think more than two or three cases could have been cured, had all other remedies been employed, and venesection omitted.

But every one who has seen yellow fever knows, that there are cases over which medicine has little or no power, and which may be pronounced fatal almost from the commencement.

In some the brain and whole system had received such a shock, the healthy functions were all so completely arrested, and the excitability to medicines so entirely lost, that they acted but imperfectly on the patient, and not at all on the disease.

Only two cases occurred while descending the river—and none after we put out to sea. On going to Trinidad, we found that island, Barbadoes and Martinique, had been healthy. A few of the crew of the John Adams had a slow bilious fever.

Doctor Samuel D. Forsyth, a medical gentleman resident at Angostura, from whom I received much kindness and assistance, took a passage with us to the United States, and was seized with the fever on our arrival at Port Spain. He was inured to the climate, had the disease once before, and except inordinate vascular action, all the symptoms were now pretty severe. He was plentifully evacuated by saline and mercurial cathartics, and calomel was afterwards given in doses of six grains every four hours. At the end of twenty-four hours, as I was giving him some drink, I perceived by his breath, that the mercury had affected his system: we were both much rejoiced. The next day his gums were swelled a little, the pains had left him, and he had a speedy recovery.

The Nonsuch proceeded to Buenos Ayres, and the John Adams to the United States. No case of fever occurred on board either of the vessels.

Although close and continued intercourse took place between the officers and crews of the two vessels previous to separation, the disease was in no instance propagated, and

the principal nurses, and some who had not been at all on shore, escaped the disease.

The river having risen nearly to its highest point, and having swept away, instead of lodging vegetable matter, to which the sources of the disease have commonly been attributed, it became difficult to trace, with any degree of precision, the places from which the disease emanated: but that there were foci in the vicinity in which the corruption of the atmosphere was elaborated, appears sufficiently evident from the cases of the men, who had been employed in cutting grass.

An opinion is entertained by several of the most eminent English practitioners in the West Indies, and which has been embraced by some in the United States, that the yellow fever may be occasioned through the agency of a tropical sun, independently of any vitiation of the atmosphere. That it will produce a fever resembling yellow fever, when in its first stage it has an inflammatory type, and is accompanied with much re-action, cannot be disputed: but it is very doubtful, whether the same malignity, and black vomit, ever attend fevers at sea, where no communication has been had with a land atmosphere, and when the vessel is perfectly clean.

The fever, from mere vicissitude of temperature, I saw in the winter of 1819. In February, the John Adams sailed for the West Indies, and after having been out a few days, the crew began to suffer by the transition from severe winter to fine summer weather. About twenty pretty severe cases of inflammatory fever appeared, all of which were checked by timely evacuating treatment. We arrived at Jamaica in sixteen days, where the thermometer was 80° Fahrenheit and upwards, but no yellow fever occurred. The ship cruised round the bay of Honduras, off Campeachy and Vera Cruz, and went into Havanna;—the crew remaining perfectly healthy.

ART. II. *Experiments and Observations on the Absorption of Active Medicines into the Circulation: and also, on the Effects of Nutritive Enemata.* By ROBERT MILNOR, M. D.

"We ought, in every instance, to submit our reasoning to the test of experiment, and never to search for truth, but by the natural road of experiment and observation."— LAVOISIER.

IT has long been believed, that the introduction of foreign matters into the blood, would be productive of death. Even milk, which so nearly resembles blood in its chemical composition, has been ranked among the most detrimental agents when applied in this manner. We may here take occasion to animadvert upon the carelessness of experimentalists, by which many errors have crept into our theories.

It is a rule in philosophy, that "similar causes under similar circumstances produce similar effects:" the circumstances then attending the operation of a cause do modify the effect, or really become a part of the cause.

It is from inattention to this rule, that hasty inferences are drawn from facts, and that so much analogical reasoning, so fascinating to the human mind, is built upon a partial foundation. This is a subject, however, upon which it is not necessary to dwell. The history of every speculative science, especially medicine, abounds in such flagrant examples, that every half-read student has smiled at the absurd vagaries, which have arisen from the results of experiments, influenced by circumstances either not known or duly appreciated.

The injection of milk into the blood-vessels is a case in point. This fluid, if suddenly introduced in considerable quantity, has a fatal tendency: but when gradually injected in small portions, it does not interfere with the natural functions of the animal, as I have proved by experiment.

Here, then, is a circumstance, the importance of which, is not apparent by any "à priori" reasoning, but which has, nevertheless, such influence upon an experiment, as wholly to change the result. Perhaps there is no substance, which may be *suddenly* thrown into a vein with impunity to the

animal—whereas, on the other hand, many substances may be exhibited in this way, in considerable quantities, without a fatal consequence, if they are gradually introduced.

This fact may be accounted for. The contraction of the heart is supposed to be effected by the stimulus of the blood. The blood may be capable of producing this effect, when somewhat diluted with foreign matter. But diluted to a great extent, its power may be too much impaired to accomplish its peculiar effect upon the heart. Now, if we suddenly inject a large quantity of any fluid into a vein, it is only reasonable to suppose that all, or a great proportion of it will enter the right auricle of the heart at the same time, to the exclusion of that blood which is necessary to excite or sustain the action of the heart. The result is obvious.

Haller observes, that substances thrown into a vein, produces certain determinate effects, as nausea, catharsis, or intoxication.* This fact throws some light upon the question of the absorption of medicines: nor is it any proof, that a substance is not absorbed and taken into the circulation, if we fail in our attempts to detect it in the blood. Dr. Seybert actually injected carbonate of ammonia into the circulation, which eluded all the tests to which he subjected it. Hence, the question is proposed by him, does the blood possess the power of assimilating substances injected into it?

I do not, however, see the necessity of this inference in the case of ammonia—for it is well known, that the serum of the blood interferes with the operation of those tests which depend upon chemical affinity. The hydrocyanate of potash, for instance, so easily detected by the sulphate of iron in a watery solution, will not produce a blue colour in the serum, unless a large quantity be present. From the analogy between the effects of certain articles introduced into the stomach, and injected into the blood-vessels, I have been induced to believe that many medicines do pass un-

* Elements of Physiology, vol. i.

changed into the circulation. The following experiments will show the effects of several articles injected into the blood-vessels.

Experiment 1st.—Having laid bare the jugular vein of a cat, I injected therein about three grains of gamboge in watery solution, at the temperature of the blood. When I had secured the vein by ligatures, and united the wound by adhesive straps, and released her from her confinement, no apparent uneasiness was manifested by the introduction of this drastic purge into the circulation, during the half hour I watched her: she was now placed in a cage. The next morning her stools were frequent, watery, and of a yellow colour, having hitherto been of a hard and natural consistence. This change was evidently occasioned by the action of the article thus administered.

Experiment 2d.—I dissolved fifteen grains of tartar emetic in half an ounce of warm water, and injected it into the left jugular vein of a full grown terrier dog. In two minutes he vomited profusely, and subsequently appeared greatly oppressed, breathing shortly—food was offered him, which he refused: in twenty minutes a violent vomiting and purging commenced—he had now become very weak, and his fæces came involuntarily from him. He expired in about thirty-five minutes.

Experiment 3d.—I injected into the right jugular vein of the same animal that served as the subject of my first experiment, five grains of gamboge in watery solution. No immediate effect was observed, but the subsequent frequency of her stools, somewhat changed from their natural colour, indicated an action in the intestines, for which there was no apparent cause but the injection of the gamboge.

Experiment 4th.—With a view to ascertain whether a small portion of tartar emetic would produce emesis, when thrown into the blood-vessels, without much disturbance to the system, I secured a cat, and having exposed the jugular vein, I injected therein half a grain of tartar emetic, dissolved in half an ounce of warm water. She did not appear

to feel any uneasiness other than that produced by her wound. She refused food and was inclined to sleep: the next morning she took food as on the preceding one, and was as lively as ever.

Experiment 5th.—I injected into the jugular vein of a small dog, a solution of one grain of tartar emetic in three drachms of warm water. Thirty minutes elapsed before he exhibited any unusual symptom, after which time he vomited, and his bowels were subsequently relaxed.

Experiment 6th.—I injected half a grain of gamboge in watery solution, into the left jugular vein of a rabbit. The animal ate some celery leaves which were given it shortly afterwards, when it was put into an apartment with another: no effect was observed to take place from this injection.

Experiment 7th.—Into the left jugular vein of a dog, I injected one grain of gamboge, and a quarter of a grain of Aleppo scammony suspended in half an ounce of warm water. He passed a natural stool immediately upon being released from his confinement. He was restless for a short time. After the lapse of half an hour he resumed his natural playfulness, but was soon affected with active purging and vomiting.

Experiment 8th.—Being desirous of ascertaining the effects of the hydrocyanate of iron on chyle and the osseous structure, I exhibited it to a cat. Thirty grains were given to him at the first dose, disguised in meat, and which, producing no effect, after the lapse of two hours the dose was doubled, and in one hour afterwards sixty grains more were given, but without any apparent effect. In about eighteen hours afterwards, I gave him seventy-three grains more, which produced nausea, as indicated by his efforts to vomit. For two hours he refused food, after which his appetite returned, and some time afterwards, he had two watery stools, which were similar in colour to the Prussian blue. I now weighed four drachms of the article reduced to a powder, and by means of thin slices of meat, tempted him to eat about one half. On the next day, I offered him more in the same manner, which he refused. I now weighed two ounces of

indigo, and tried him with that article in small quantities, which he took, when it was disguised in the same manner: in feeding him from time to time, I administered the indigo until the whole was taken, which was accomplished on the thirteenth day from the commencement. He was then killed, and on examining the chyle, found it was slightly tinged with blue, so as to be easily distinguishable from natural chyle. His bones, also, exhibited this colour in a remarkable degree.

" Hence," as a celebrated writer* informs us, " a great many substances may enter the lacteals along with the chyle, even solids reduced to a fine powder.

" When indigo has been thrown into the intestines of a sheep, I have seen the chyle rendered quite blue : now, indigo is not soluble in water, but is a solid reduced to a very fine powder. So, musk gets into the chyle, giving it a strong smell, and a great variety of other substances of various colours, various tastes, various smells, each of them giving colour, taste and smell to the chyle."

Dr. Musgrave† injected a solution of indigo and stone blue into the small intestines of dogs, which he had kept fasting for a considerable time. On opening them some hours after, he discovered that the lacteals and thoracic duct were coloured with the injections. Thus, it appears, that the colouring matter of Prussian blue and indigo may be taken up by the lacteals and deposited in the bones.

Experiment 9th.—Having rubbed down in a mortar one drachm and a half of assafœtida, with half an ounce of warm water, I secured a half grown dog, and having exposed the left jugular vein, injected it therein. A considerable degree of debility and nausea immediately ensued. In ten minutes he vomited, and in the course of an hour he had two evacuations, one of which was of the natural consistence. Half an hour afterwards, I drew one ounce of blood, which was impregnated with the odour of the assafœtida.

Experiment 10th.—Having laid bare the right jugular

* Fordyce on Digestion.

† Philosophical Transactions abridged, chap. 4. part 2. page 76.

vein of a small bitch, that had taken rhubarb for several days in meat, I injected therein five grains of gamboge, dissolved in one ounce of warm water. In the course of seventeen minutes, she had two stools, one of which was of natural consistence, the other fluid. So great was the prostration of strength, that she staggered and fell, when she attempted to walk. She continued much oppressed with nausea and purging, and in three hours and a half died. I collected about one drachm and a half of urine, having tested it with potash, and found rhubarb present.

Experiment 11*th.*—Having starved a dog for three days, I commenced feeding him with madder enclosed in thin slices of meat, which he ate voraciously. I continued thus to feed him, until he refused to eat, and pursued this course several times a day for ten days. He had now consumed one pound and a quarter of that article, and had become much emaciated, and extremely costive and thirsty, drinking so immoderately as to occasion pain from over distension of the stomach, with great lassitude denoting sickness. Finding that I could push the madder no further, the idea occurred, that if he would take annotta, it might answer the purpose of madder, and from the saccharine matter with which it is compounded, afford him nourishment. His change of diet proved agreeable, for whether combined with meat or alone, he ate it readily, and in two days consumed half a pound. This produced, on the next day after eating, an altered condition of his bowels: they became laxative, resembling in appearance Spanish brown paint. Anxious now, to ascertain the state of his bones, and having been led to believe that milk (though most analagous to blood of all the secretions) if thrown into the blood-vessels would kill, he became the subject of an experiment to determine this fact. Having him well secured, I exposed the right jugular vein, and having in readiness two ounces of fresh breast milk, I injected it gradually into the circulation, secured the vein and released him, and watched him for three hours and a half. Not the slightest disturbance was apparently produced on his system. I tendered him some meat,

which he ate, and then left him. The next day found him well as usual. I now opened the left jugular vein, and threw in one ounce of cow's milk that I had previously warmed, after which I offered him some, which he drunk and layed down and slept: on entering the room two hours afterwards, found him lively. I now had him again secured for a repetition of the experiment with skimmed milk, two ounces of which I injected into the last mentioned vein. I afterwards offered him some food, which he refused—and in half an hour tendered it again, and he ate it. I now had him killed, and found the intestines much distended with annotta and madder. On examining the chyle of the thoracic duct, it was tinged with the article given, and the bones of a light pink colour, which on boiling, assumed a dark reddish hue. The venous blood was almost the colour of ink, whilst the arterial was of the usual florid appearance. Thus, I trust, I have satisfactorily proved, that milk, as well as other articles, may be thrown into the circulating fluids with impunity.

Experiment 12*th*.—I fed a small dog on garlic enclosed in thin slices of meat for sixteen days, when I drew some blood from the jugular vein, in which the odour of the garlic was very sensible. I now injected into the vein one ounce of blood taken from another dog, which appeared to have no effect upon him. I then left him, and on the afternoon of the same day had him killed: the whole animal, on examination, seemed saturated (if I may use the expression) with the odour of garlic. To a hungry dog, I offered some of the meat, which he took in his mouth, but dropt it, and after smelling turned away from it. I then offered him a piece of the flesh of another dog, which he ate voraciously. To a cat I gave a small piece, which she ate reluctantly—a second piece was offered, which she refused, although hungry.

Experiment 13*th*.—With a view still further to test the vital powers of the blood, in disposing of, or assimilating to itself articles when thrown immediately into the blood-vessels, I dissolved half a drachm of carbonate of ammonia

in half an ounce of warm water, and injected it into the jugular vein of a large cat, which caused a prostration of strength, accompanied by cries indicating pain: it produced nausea, but no vomiting. Food was offered, which she refused. I drew about one ounce of blood, shortly after, and tested the serum with litmus paper, without detecting the presence of the alkali: she slept during the day, and the next found her well as usual.

Experiment 14*th*.—Having procured some breast milk, I placed it in warm water, to preserve the temperature, and next exposed the right jugular vein of a dog, and injected therein one ounce. No uneasy sensation whatever was manifested.

The next I day exposed the left jugular vein, drew one ounce of blood, and mixed therewith four grains of calomel, and immediately threw it into the circulation, secured the vein and liberated him. He staggered in walking and appeared oppressed—his breathing was short and quick—he lay down, again rose to walk, and in ten minutes vomited, by which he was relieved: in forty-five minutes he had two stools, one of which was of thin consistence, when nausea and gastric distress abated: he slept, and kept quiet the greater part of the day, and on the next morning took food as usual.

Experiment 15*th*.—I fed a small bitch on indigo disguised in meat for ten days: when she had taken two ounces and a half of the indigo, her bowels became very torpid. She voided it in lumps, apparently unchanged. Her bowels resumed their natural state, in which they continued for three days, when she died. On dissection I discovered a rupture of the gall bladder. The liver exhibited a yellowish appearance, very different from any that I had heretofore seen. The mesenteric lacteals were distended with chyle of a light bluish tinge, the bones also were spotted in many places with the indigo tint.

Experiment 16*th*.—I concealed rhubarb in the meat with which I fed a dog for three days. Its cathartic operation rendered him very weak. He refused any longer to eat the

meat which contained the drug. I forced him to swallow a quantity of a strong infus. rhei, in an hour and a half, after which I killed him, by opening the thorax and allowing the lungs to collapse. The chyle in the thoracic duct exhibited no unusual appearance. There was, however, only a small quantity of it collected, perhaps not sufficient for a fair examination. The arteries were empty, and the veins extremely distended. I collected about one drachm of bile, and one drachm and a half of urine, in each of which I detected the rhubarb, by means of potash.

Experiment 17*th*.—With a view to ascertain the effects of odoriferous substances, I commenced with a dog that had been starved for two days, and having before me a vial of musk containing two scruples, I enveloped a small quantity of it in thin slices of beef, which he readily swallowed—so that in sixty hours he had taken the whole quantity. He was then killed, and some chyle was collected from the thoracic duct just before it enters the subclavian vein—but there was no evidence of the article there. I now extended my researches further, and punctured the vena portæ, and found the odour of musk very sensible there, as well as in the pelvis of the kidnies, and the ureters. There was no urine in the bladder, but the odour was present.

Experiment 18*th*.—With a view of ascertaining to what extent the intestines are adequate to the assimilation of food, independently of the stomach, I instituted the following experiment.

Dec. 1*st*, 1821. I procured a large cat, and starved him for three days, and now ascertained his weight to be five pounds four ounces.

5*th*. Placed him in a large cage, after administering per anum, one gill of beef soup—found he retained it, and in fifteen minutes injected half a pint more. At noon repeated the last injection.

6*th*. This day the enema was exhibited twice, half a pint each time.

7*th*. Repeated the enema twice.

8*th*. It was repeated three times.

9*th*. Gave the enema twice.

10*th*. Exhibited this day three half pints of veal soup.

11*th*. Placed him in the scales, and found his weight to be four pounds and twelve ounces, losing in six days eight ounces in weight. The soup was given during this day twice, half a pint each enema.

12*th*. It was repeated twice, half a pint each time.

13*th*. Three half pints were given during the day.

14*th*. Provided a richer soup, and gave two half pints this day, and placed before him a cup of water, which he drank.

15*th*. Three half pints were given this day. During his confinement he had few stools, and small in quantity.

Satisfied so far, in the progress of the experiment, that the wonderful powers of nature were exercising their influence in this case of exigency to sustain the vital functions—I placed a muzzle over his mouth of sheep skin, secured around his head, with openings for the eyes and nose, and enlarged his confinement from the cage to the room.

16*th*. Three half pints were administered this day.

17*th*. Three half pints do. do.

18*th*. Three half pints do. do.

19*th*. This day he had one half pint of soup, there being no more prepared.

20*th*. Desirous of confirming the latter part of experiment eleventh, I secured him, laid bare the right jugular vein, and injected into the circulation one ounce of warm cow's milk. After securing the vein, I closed the wound with adhesive plaster and released him, placing him on the floor: there was much prostration of strength, and uneasiness evinced by his cries. I gave him a gill of water, which he drank, and appeared revived. The usual enema of soup was next administered, and in a few minutes he evacuated a portion of it: he was now placed in the cage with openings at the bottom, in case any should be evacuated, it might run off, and his muzzle was removed, it being a source of uneasiness. In three hours afterwards the injection was repeated, and he was watched for one hour, and he retained it.

21*st*. Two half pints were administered.
22*d*. One half pint was given.
23*d*. Two half pints of mutton soup were given.
24*th*. Three half pints do. do.
25*th*. Two half pints of chicken soup do.
26*th*. Three half pints do. do.
27*th*. Three half pints do. do.
28*th*. About one gill and a half were given.
29*th*. Nothing given this day.
30*th*. Two half pints of chicken soup were given.
31*st*. Placed him in the scales, and found him to weigh three pounds and two ounces, losing in twenty days one pound and ten ounces. The injection was administered twice this day, the usual quantity of half a pint each time.

Jan. 1*st*, 1822. Three half pints of rich soup were given this day.
2*d*. Three half pints do. do.
3*d*. Twice the injection was exhibited this day, half a pint each time.
4*th*. Three times this day, the usual quantity each enema, and gave him some water to drink.
5*th*. No enema was administered, now weighed him, and found his weight to be three pounds.

Here I closed the experiment, satisfied with the result, and gave my prisoner better fare. Thus was the life of an animal, accustomed to daily food, sustained for one month by alimentary substances injected per anum. It cannot be made apparent, that the stomach in this case exercised any influence upon the articles appropriated to the sustenance of life. The duration of the experiment, which might have been extended, was sufficient to prove that absorption from the intestines may take place without the usual routine of the digestive process. Hence, it may follow, that certain medicinal substances introduced into the stomach, may resist the action of that organ, which would tend to their decomposition, and escape into the inferior portions of the alimentary canal, whence they may be absorbed and carried

into the circulation, where they exhibit their effects on the animal economy.

The analogy between the effects of some articles introduced into the stomach, or thrown immediately into the circulation, emetic tartar for example, corroborates the idea which is here held forth. Besides which, it is well known that some substances, after having been introduced into the stomach, are found in some remote parts, to which they could only have passed through the medium of the circulation.

The colouring matter of vegetables is deposited in the bones, the sensible properties of certain odourous substances are evident in the secretions, and globules of mercury have been found, after an exhibition of certain preparations of that metal, in various parts of the body, between which and the stomach, no other communication exist, than the indirect course of the sanguiferous and absorbent systems. Nor is the fœtus in utero exempt from the influence of certain agents introduced into the system of the mother, either through the stomach, the lungs, or by inoculation. "There are positive facts," says Swediaur,* "to prove that the venereal poison as well as mercury, is taken into the circulation. An infected father has been known to communicate the disease to the fœtus, when he had no indications of it about the genitals."

Children have also been born with the small-pox, having received it from the mother during their gestation.† Mr. Turnbull relates the case of a lady who was inoculated in the seventh month of her pregnancy—nine days after the eruption, she received a fall, and in a few days after that was delivered of a dead child, which was covered with variolous pustules, and a state of suppuration." The matter was proved to be variolous from its communicating the disease to several persons who were inoculated with it.‡ "If the poison of contagious diseases can circulate in the san-

* Swediaur, tom. 2, page 10. † Foote on the Venereal, 4to. page 468.
‡ Memoirs of London Medical Society, vol. 4. page 364.

guiferous system, why may not medicines, which are not more active, also pass into the system."*

It is not intended to argue, that the absorbents take up every substance that may be presented to them in the intestines. On the contrary, they may exert a discriminating faculty of selecting those parts of the food which are applicable to the wants of the system, and rejecting in most cases the useless and pernicious. From the circumstance also of animals being supported for a considerable length of time by injections of nutritious matter *per anum*, it would appear that they are endowed with a digestive power; for in such cases we cannot suppose that the gastric fluid, the bile, or the pancreatic juice has any influence: it must, at least, be very inconsiderable. Among the numerous cases illustrative of the above, are those recorded by Home,† Haller,‡ and Fourcroy,§ which sufficiently attest the fact, that the intestines possess the powers alluded to.

The modus operandi of medicines is one of those secrets of nature, which the ingenuity of man has hitherto vainly endeavoured to expose: nor is it probable that a theory will ever be established, which will embrace all of its phenomena. By careful observation, however, and a proper degree of caution against the fascinations of general principles, we may gradually dissipate much of the gloom in which we are enveloped in this field of inquiry. The idea that medicines are taken into the circulation previously to their operation on the system, or that they are carried by the blood to particular parts, where their action is desirable, is not now for the first time suggested. It was the basis upon which the humoral pathologists founded their doctrine. I am aware of the interest which this important subject has excited, and of the discussions to which it has given birth.

I cannot undertake, in the present state of our investigations, to express a decided opinion, whether all medicines

* Smith on Absorption.

† T. Home, Medical Facts and Experiments of 1752, page 87.

‡ Haller's Physiology, vol. 7, page 178.

§ Fourcroy's Medical Illustrations by Sciences, 4. 103.

exhibited internally, do or do not enter into the circulation, as a previous and necessary step to those effects of which we know them to be productive. But that some articles do enter the blood, seems to be demonstrated by their appearance in the secretions. That foreign substances may be introduced into the blood-vessels with impunity to the animal, I have also ascertained by actual experiment—and moreover, that active substances produce effects upon the system when thus introduced, similar to those which result from their exhibition by the mouth.

ART. III. *Historical and Critical Observations on Syphilis.* By A. J. L. JOURDAN, M. D. Translated from the French, by R. LA ROCHE, M. D.—(*Continued from No.* 6.)

CHAPTER V.

OF all others, Nils De Rosenstein contributed most to the propagation of the doctrine of latent venereal, whose work, translated into most of the languages of Europe, remained for a long time the most distinguished on domestic medicine. He extended this doctrine to its utmost limits, and in its support brought forward observations of so singular a nature, that we think it necessary to lay a few of them before the reader. Two individuals had enjoyed good health until the thirtieth year of their age, when they both became affected with venereal symptoms—who attributed them to the *milk received from their nurse*, and (to their great astonishment no doubt) were implicitly believed by Rosenstein. A child was cured of the crusta lactea, and of aphthæ of the mouth, by means of mercury—which led the Swedish physician to suppose these diseases to have been venereal, proceeding from the nurse, who whilst weaning the child, passed its nourishment in her mouth. *Risum teneatis!*—If the virus is thus transmitted from parent to child, without af-

fecting the first, it is because when absorbed immediately *(d'emblá)* by individuals of too corpulent a form, it does not produce on them, during the whole course of their existence, any visible symptoms, but passes directly with the blood of the mother into the body of the fœtus. Being very acrimonious it kills the embryo; but when less so, the fœtus arrives at its full period, and its health is not deranged for a space of time more or less considerable, for every chronic disease that refuses to yield to usual treatment, must be attributed to a latent virus. This disease, which Lieutard, and after him every one else, called a Proteus, takes on every imaginable form and shape. It is especially in corpulent habits that it more frequently remains in action, without giving any evident signs of its existence—but should there occur any general revolution in the system—any acute diseases—should the person make use of chalybeate waters, or other preparations of iron, or strong liquors,—should he abandon himself to violent passions, to excesses in the pleasures of the table, or immoderate exercise, or finally, should he pass from a cold to a warm climate, then the disease makes its appearance, and manifests itself under the form of syphilitic symptoms more or less distinct, complicated, or disguised.

Nor was this all—these ideas, however extraordinary, were soon regarded as too simple, and as one step more into the field of hypothesis, appears nothing when we have once entered into that boundless space—it was generally admitted that the continued presence of the virus mitigated only by remedies, is a powerful cause of the physical and moral degradation of the human species. Rosenstein did not hesitate to support this monstrous absurdity. He was however, surpassed by Sanchez, who maintained that the venereal disease will sooner or later, so affect the human race, as to become the cause in Europe, at some future day, of a revolution similar to that which overturned the Roman empire. Theiri who followed, was also drawn into this error—" It is true, he says, that the venereal diseases which is inherited by children, does not communicate to their in-

fection—but the alteration of the germs is shown by the weakness of the organs, especially the brain, as is rendered evident by their want of application, by levity of their mind —by the little consistency of their ideas and character—I have also perceived that it often diminished the fecundity of the sexes."—As it is a general rule (which, however, counts as many exceptions as cases,) that persons who have been several times affected with the venereal disease, have abandoned themselves to immoderate venery, it was natural, from the reigning theories of the times, to attribute to this disease, the moral and physical debility of their offspring, whilst the true cause of their effects were to be looked for in the decay of the generative faculties, and in the effeminacy and wasting of body, caused by excessive venery.

To give therefore, the most formidable aspect to the phantom that had been raised, it was not thought sufficient to have disturbed the peace of such as had already experienced its wrath; but no less terror was created in those not directly affected, by an impression which was studiously inculcated, that its effects might be extended to them, through the misconduct of their parents.*—Rosenstein did not hesitate to affirm, that those who have had chancres which suddenly disappeared, or were cured by local means—or those who have been affected with blennorrhagia, which continued too long, or was cured too soon, and those in whom the inguinal glands have been indurated—however healthy they may appear, are nevertheless ill, *and require mercury to restore them to a natural state* of health. From this arose

* We find in one of the most esteemed modern works on syphilis, that a man affected with a syphilitic ulcer in the throat, had, during the treatment, connection with his wife, who never before, nor ever afterwards had any symptoms of infection---the child was attacked a few weeks after his birth, with an ulcer in his throat, exactly in the same part as was situated that of the father. This coincidencc in the nature and situation of a symptom of the disease the most irregular of all in the productions, of the numberless accidents attributed to its action, is sufficient, by itself, to render doubtful the whole anecdote, even in the mind of the most zealous proselyte to the present theory of syphilis.---Quod probat nimis probat nihil.

the advice he gave to those about to be married, of examining scrupulously into their conduct, so as to be assured that they are really not affected, or else have been radically cured; and in case of the least suspicion, to recur, as a precautionary means, to *mercury*, though there exist no indication to authorise its employment, and notwithstanding too, that it has been demonstrated beyond all possibility of doubt, even by the most zealous partisans of the present theory of the venereal disease, that mercury exercises no specific action in the case. But how shall we reconcile this assertion with the supposed impossibility of completely destroying the virus in the system, which has given birth to another precept no less absurd, that of making use, as a *precaution*, of a certain number of frictions, and of continuing mercury for two or three weeks after the total cessation of all venereal symptoms. These absurd ideas, which would be rejected perhaps by Purgon, had,* even at that period, met with many supporters! and are consequently found in modern works, commendable in many other points of view Let us say with Pliny, *Didicimus periculis nostris, et experimenta per mortes egimus.*

The theory of latent venereal diseases, consequently made rapid progress after the time of De Rosenstein. It is true, that Gerard Van Swieten restrained it somewhat, in refusing to admit the hereditary quality of the virus. But by Maximilan Stoll it was adopted in the fullest extent. John Frederick Consbruch believed gutta serena to depend on syphilis, an hypothesis never before advanced, and finally appeared the first book *ex professo* on these affections, written by Stephen Hausmann. Peter Fabre following the traces of his master Petit and of Rosenstein, spared no pains to demonstrate their real existence, and even extended the doctrine still farther than had been done by the last mentioned author, in saying that *the venereal virus is capable of*

* Mr. Purgon is the name of the physician in Moliere's comedy, of the Malade Imaginaire.

producing all chronic diseases; that whenever a malady resists well chosen remedies, its presence must be suspected; and that the most distant suspicion of its existence, warrants us in administering anti-syphilitic medicines. Augustus Frederick Hecker, combining the ideas of his predecessors on the progressive degradation of the human species from the influence of syphilis, believed, as had before been done by C. G. Hufeland, that this latter differs in nothing from scrofula, or rather, that this last had degenerated from the former.

In the end, physicians, after for a long time copying each other, dared to contemplate the idol to which they had been sacrificing, and gradually it lost a part of that veneration it had inspired. The able successor of Van Swieten, John Andre, attacked the doctrine of latent venereal disease in the most determined manner, and proved that it had arisen from the ignorance of the causes, and from the treatment of those maladies classed under that imaginary denomination. Neither did Bosquillon spare this theory, and his judicious mind led him to the following reflections. "I am inclined to regard all that has been said on the subject of hereditary venereal virus, as the product of an ardent imagination or of ignorance. It appears proved, that this virus does not leave any effects behind, which, remaining latent in the body, only appear at the age of puberty, or in the decline of life. Those who admitted this hypothesis, have mistaken diseases of a particular temperament for symptoms of the venereal virus." In the first edition of his work on the venereal disease, Girtanner totally rejected this singular hypothesis, and was in this respect imitated by Frederick Ferdinand Bretschneider, who, however, did not push his scepticism quite so far as Girtanner had done, for he still admitted the existence of certain *chronic* venereal diseases, which epithet had, by Sanchez, been substituted for that of latent. On the other hand John Steiglitz, with the intention of putting an end to the disputes existing between the two parties in Germany, thought it necessary to range all latent venereal diseases under three distinct heads. The first class, which he regards as absurd, and contrary to true philoso-

phy and to observation, comprehends those diseases derived from an hereditary virus. In the second class, the virus, though present, does not manifest itself by any symptoms evident to the senses. Steiglitz admits the possibility of these cases. The third class is constituted of diseases, which, though produced by the venereal virus, are not supposed syphilitic, since they are not the usual product of this virus, and may arise from some other morbid cause. Girtanner, with some foundation, objects to the second class, that there cannot be said to exist a latent venereal disease, when the virus existing in the system does not manifest itself by some evident symptom. Even, indeed, admitting the existence of this virus, it would be less our object to ascertain whether the poison really exists in the system, than that it has deranged or altered its functions. But, had Girtanner, who overthrew so many errors, been less the slave of prejudice, he might readily have made use of an argument still more conclusive. How can we credit the existence in the mass of humours, more especially of the blood, of a venereal virus, when, even admitting that it really exists, and is introduced in the lymphatic system, it cannot help receiving, together with all other products of cutaneous and intestinal absorption, whilst in the lungs, the altering action of the atmospherical air? It is conceded, that the materials for secretions, do not pre-exist in the blood, but only their elements, and notwithstanding this, it is maintained that the arterial blood in its course through all the parts of the system, is accompanied by the venereal virus! Indeed, some have lately gone so far as to attribute all internal aneurisms to the action of this virus on the arterial coats; which, however, are only supposed to become altered, when, by some obstacle, the circulation becoming impeded, this virus is obliged to sojourn some time in the same place. This very strange hypothesis leads us to remark, that long before this period, George Wofgang Wedel and Andrew Loew had recommended the transfusion of blood, for the cure of syphilis.

Misled by the well written work of Steiglitz, Girtanner

modified his first opinion, and in the new edition of his treatise, maintained, that there exist latent venereal diseases of the third class. It is, however, clear that these have merely been admitted à priori, for they naturally arise from the adoption of the new theory of syphilis—but the difficulty is to prove their existence, and to assign them their distinctive characters. The only one that has been given, is their cure by means of mercury.* But were we to receive this as a proof of their venereal character, then might we with as much reason, place in the already too extended domain of syphilis, small-pox, inflammations, intermittent fevers, scrofula, &c. for the cure of which, mercury has been advised and employed with success—at the same time, we should be obliged to exclude from that class, a number of maladies, at the present time considered syphilitic, but which resist with obstinacy mercurials, and are even aggravated by the use of these remedies. It is very well observed by Bosquillon, that the method of cure which succeeds, is not sufficient, under any circumstances, for establishing the true nature of the disease.

Nevertheless, the abuse made of the theory advanced by the physicians of the middle ages, and the errors to which it had gradually led, caused many to meditate seriously on the practice which time had established. These reflections raised doubts as to the real existence of this frightful hydra, which under the name of venereal disease, had become the torment of physicians, and the terror of patients, disposed to recognize in all affections some transmutation of this new Proteus.

In the year 1767, Francis Balfour opposed the opinion,

* The author of the new etiology of aneurisms, has reasoned in this manner. Having cured several aneurismatic tumours, by the use of mercurial frictions, applied to them, and also on the whole surface of the limb, he concludes that aneurisms are almost always to be attributed to the venereal virus, and that the internal administration of mercury, is always preferable to its external application. But in all the cases however, he relates, he had the attention of applying simaltaneously with the mercury, a compressive bandage.

that blennorrhagia depends on syphilis.* William Ellis embraced his opinion, which was afterwards more fully developed by John C. Fede, and Andrew Duncan, but more especially by Benjamin Bell, who extended it so far as to convert an assertion, until then dubious, into a fact absolutely certain and evident. Notwithstanding the efforts of John Andrews, Ernest G. Baldinger, Samuel Foart Simmons, John D. Metzger, Cullen, Hunter, and William Harrison, Bell caused a general revolution all over Europe, with the exception however, of France, where, from some unfortunate circumstances, prejudices on the nature of syphilis, seem to have continued, and where the old error, that blennorrhagia is produced by the venereal virus, is still almost universally believed.†

To have taken blennorrhagia from the domain of syphilis, was undoubtedly to have already effected much: it was in fact depriving it of its chief appendage. But this was not all, and chancres were also withdrawn from its empire, though in truth, in a manner far less positive—so true it is, that to shake off the opinions of the times, proves always

* Before Balfour, Boerhaave had, in 1751, compared blennorrhagia to the coryza, and said that the matter discharged from the urethra resembles that secreted by inflamed pituitary membranes.

† We still find practitioners repeating with Fabre, that the blennorrhagic discharge often resists frictions, but that *notwithstanding this circumstance*, we may morally assure the patient of the radical cure of the syphilis. It is curious to see a physician, satisfied with the belief, that he has cured a disease, existing only in his imagination, and at the same time attempting to remove the fears which the patient may have of a real malady, which far from being cured, is even aggravated. Another prejudice which exists on the subject of blennorrhagia, is that the general infection is little feared so long as the discharge is copious, and unmixed with blood; but should any mechanical or other cause, produce a rupture of the vessels of the lining membrane of the urethra, then the absorption of the virus is feared and mercury is administered. Is this absorption then more threatening after than before this excoriation? Do not the absorbents act with as much energy in both cases, and even with more force in the second? What a contradiction to believe that this absorption does not take place in the urethra so long as its internal membrane remains entire, whilst it is supposed it can be affected without the least excoriation on the glans penis, in those cases when the disease is said to be taken *immediately (d'emblée.)*

of the utmost difficulty. William Cockburn was the first, who aided by reasoning and experience, proved that chancres can with safety be cured by local means alone, as the general affection seldom follows this treatment.—What is very curious, Astruc held nearly the same opinion! Though he considered chancres as the principal inlet of the virus into the system, he assures us that in many cases they heal of themselves, without the least syphilitic symptom affecting afterwards the patient. This forms another of the numerous assertions of this author, which appear incompatible with the rest of his system. When a man endeavours to establish a doctrine which appears contrary to nature and common sense, it requires so scrupulous an attention, to accommodate to it all the facts already known, that sooner or later he forgets himself, and permits some sparks of truth to break through, notwithstanding all his endeavours to the contrary. Girtanner affords himself, as another proof of this fact—no one more than he insists on the necessity of the local treatment.* He maintains that the constitutional infection seldom succeeds to chancres treated in this way—but as the doctrine he defended, led him to fear its occurence, he advises as a precaution, mercurials to be administered internally—*for if they are of no benefit, they at least do no harm!*

The edifice of the venereal disease, which, for its erection, had required more than the space of three centuries, was now shaken even to its very foundation. Blennorrhagia and

* Girtanner advises as the best mode of curing ulcerations of the genitals, a weak solution of caustic potash, to be applied five or six times a day by means of a camel's hair pencil. We have frequently ascertained the efficacy of this remedy. It will not however succeed in indolent chancres, and it is well to cease the application, as soon as the inflammation it produces, has caused a uniform redness over the surface of the ulcer. Lotions with lime water, or Goulard's extract of lead, are sufficient to accomplish the cure. In painful chancres, a strong solution of opium, as proposed by William Turnbull and L. C. Althof, is employed with advantage. As soon as the great sensibility of the sore has abated, the weak astringents suffice to cicatrize it. Mercurial powders sometimes succeed, as excitants, but liquid stimulants are preferable, or more easily managed.

chancres were no longer regarded, except as mere local affections, susceptible of cure by purely local remedies. One step more, and it would have been totally overturned, since no other inlet was left for the introduction of the virus into the system, than by direct absorption, and although physicians obstinately admitted the possibility of taking the venereal disease *immediately (d'emblée);* although they answered to those who still doubted the probability of this theory, that absorption does not take place directly, but is effected in all cases from ulcerations of too slight and superficial a nature to attract the attention of those who are affected with them, the known laws of organization opposed a theory altogether in opposition to them, and raised with force against the existence of a venereal virus. For a long time past, this idea had been germinating in the minds of many pathologists. Charles Musitanus had already exhausted all the subtilties of dialectics to prove that there exists no venereal disease. Michael Aloysius Sinapius, had also attempted to demonstrate, that to a certain extent, the disease does not exist. But this writer, so well known for his propensity to paradox, lost his cause in supporting the singular opinion, that the various accidents regarded as syphilitic, are for the most the results of continence. The inconveniences of mercury, caused, afterwards, the venereal symptoms to be attributed to this remedy. We will not here insist on the opinion of John Linder, who believed that mercury destroys the generative faculties, in killing the spermatic animalculæ: nor that of Antony Agostini, who attributed to this medicine the strangury denominated venereal, because experience has proved this last, to depend on the tendency which the living membrane of the urethra has, in common with all other mucous membranes, of contracting whenever it has been inflamed. These ideas, however, became gradually more developed and arranged.

In the year 1747, A. Retter supported, under the presidency of Jerome Ludolf, a thesis, in which he advanced, that the most dangerous symptoms of syphilis are less the effects of the disease, than of the mercurial treatment.

Lewis Kornbeck, in the year 1776, developed more fully this assertion, in his dissertation, entitled, *Historia morborum à Mercurio.* Physicians went so far as to distinguish the *mercurial* from the venereal disease, and to ascribe to the former, a number of affections formerly imputed to syphilis. Finally, in the year 1811, a writer had the intrepidity to oppose openly the universally received doctrine, and to affirm, that there is no venereal disease, that this last is a mere assemblage of morbid affections of a different nature, and that the contagious virus, to which they have been falsely attributed, has never had existence. This writer is to be reproached with two things; and first, for having concealed his name: no one should ever fear to acknowledge himself the author of a book, when he thinks he has in it combated for the cause of truth. Secondly, for having written in too slight and superficial a manner; he should have borne in mind the words of the philosopher of Ferney: "*La plaisanterie n'est jamais bonne dans le geure serieux, parce qu'elle ne porte que sur un côté des objets, qui n'est pas celui que l'on considère; elle roule presque toujours sur des rapports faux, sur des equivoques.*" Too much gravity cannot be shown, when we display the deplorable consequence of errors, which merely owe their consecration to time and credulity.

We have thought that the best criticism that could possibly be made on the present doctrine, was to ascend to its very origin, and to mark down its various progressive steps. Before its invention, venereal diseases were less spoken of than at the present time; not because they were not known, but because they were not yet thought to arise from a cause susceptible of infinite variations in the form, number, character, and intensity of its effects. Excesses in the pleasures of Venus, have produced at all times, as they do at present, blennorrhagia and its baneful consequences—chancres, and other accidents arising from them—excrescences of various species, &c. As at present, they have always produced a total wasting and weakness of the powers of the system, from which naturally arose a number of cachectic affections,

so that when the venereal virus was discovered, it was not long ere these diseases, the necessary consequences of a state of feebleness and irritation, were attributed to its presence in the body. Is it necessary to recur to a specific virus, to explain all the various accidents which have been attributed to it? Surely not. We can well suppose, from analogy, that discharges or ulcerations can spontaneously arise in prostitutes, from the mere irritation to which their genital organs are continually subject, and which is further increased by their intemperance and excessive use of ardent spirits. And to account for their contagious nature, we are only to recur to the acrimonious nature of the fluids secreted by the organs where they are situated, and which, from their virulent qualities, are rendered susceptible of producing an irritation and inflammation of the same nature, when applied to the delicate membrane of the glans, prepuce, and urethra of healthy subjects. Do we not see in other cases, the altered natural secretions acquire a still greater degree of acrimony, and the mucus secreted in coryza excoriate the whole of the superior lip, the fluid discharged in dysentery denude the extremity of the rectum? Notwithstanding all this, however, no one has yet thought of a dysenteric virus, nor one of coryza. Finally, is it not attested, from numberless observations, that the mere irritation arising from the pleasures of love, independent of any other circumstances, can produce in a virgin, or in a woman who has been a widow a long time, discharges and chancres, in the same way that masturbation will sometimes cause blennorrhagia in men.*

* Discharges are more frequently met with in men than women, or rather, the delicacy of the latter prevent them from acknowledging a disease, on the nature of which, from the circumstances of its appearance, they should be perfectly secure. They are more especially common in young men of an ardent disposition, who for the first time enjoy the pleasures of Venus, and abandon themselves to excesses. Ulcerations are sometimes seen, though in truth, more rarely. But in most cases, their seat would indicate, that they arise from the tearing of the frænum, from which results a wound, that is prevented from healing as soon as it would in any other part, by the irritation it experiences from the sebacious matter. A physician who is consulted in such a case, at first smiles sarcastically, then

Charles Frederick Hundertmarck avers his having seen symptoms similar to those denominated syphilitic, arise from the venereal act between too scorbutic individuals. Chancres appearing in the throat, when those about the genitals have been suppressed too suddenly, have served as an argument in favour of the doctrine of general or consecutive syphilis. But are we destitute of examples of the remarkable and inexplicable sympathy existing between the organs of generation and the throat, which is well exemplified in the change of the voice that takes place at the period of puberty, and is also manifested, as observed by Catullus, in the swelling of the neck, which happens to virgins who have but lately menstruated? Is it not to the sympathy existing between the genital organs and the integuments of the body, and not to any specific virus, that we attribute those pustulous herpetic affections that are caused by excessive masturbation? Why, then, should we have recourse to a virus in accounting for the pustules that appear on the integuments of those, whose genitals have been exposed to a long continued irritation, and that break out sometimes with so much rapidity, that it is impossible to attribute them to any other cause than sympathy; for, according to John Andrew and other practitioners, these cutaneous affections appear in twenty-four hours after the manifestation of the primary venereal symptoms? Is it not, lastly, to sympathy, and not to any imaginary virus, that we attribute the singular phenomenon manifested by the swelling of the

answers gravely to the protestations which his innocent patient makes to him, *nemo dat quod non habet.* Is it also the woman who is to be accused of the *tabes dorsalis*, and of the true gonorrhœa, which affects the man who has abandoned himself to excessive venery? How many families have been disturbed by the exclusive and inconsiderate application of this maxim! Their happiness has often been destroyed, and can with so much the less probability be restored, that it is admitted possible a woman should infect a man, without, however, being, or afterwards becoming, diseased herself; the virus, which had only been concealed in the vagina, being removed by lotions, so that the absence of a local affection is not even a proof of her innocence, and cannot either suffice to make us doubt her crime!

parotid gland consequent on that of the testicle, or of the female organs of generation? The same thing occurs with respect to plica polonica. The Polish physicians, and among others Doctor De la Fontaine, have equally made of this latter a frightful monster, embracing in Poland almost all known diseases; so much so, indeed, that Wolframm has placed in its domain a number of symptoms hitherto considered as syphilitic. But on the other hand, the French physicians have gone much further on the subject of plica polonica than with respect to syphilis. They have totally rejected its existence, and refused to admit that of any trichomatic affection. Notwithstanding, however, all the arguments they have alleged on this subject, it is as if we refused to believe in the existence of venereal symptoms, before we are convinced that there does not exist a venereal virus. In concluding this subject, we will observe, that the important branch of medicine which relates to this latter affection, stands in need of a total reform, and that to it may especially be applied those words of the celebrated Bacon: *instauratio facienda est ab imis fundamentibus, nisi libeat perpetuò circumvolvi in orbe, cum exili et quasi contemnendo progressa.*

It is now necessary to say a few words on the subject of the fatal disease which appeared about the close of the fifteenth century, and has wrongly been considered as the source from which proceed all the actual venereal symptoms. When we compare the testimonies of the most veridical historians and physicians, we think it impossible to doubt its being derived from the *Marranes*, who were expelled from Spain before the discovery of America. Fulgosi, among others, tells us, that it originated in Ethiopia, *quæ pestis, ita enim visa est, primo ex Hispania in Italia allatâ, et ad Hispanos ex Ethiopiâ.** At that time all the parts of Spain occupied by the Moors were called Africa, and afterwards Ethiopia. Infessura, who noticed the first ravages of the epidemic at Rome, calls it *pestis Marranorum: Mortui sunt*

* De dictis factisque memorab. l. 1. c. 4.

quam plurimi ex peste et contagione Marranorum. Beniveni, Benedetti, and Fracastorius derive it from Spain. John Trithamius, abbot of Spanheim, likewise informs us, that it originated in that country: *habet suæ infectionis pestiferæ principium in Hispanis.** The period of its appearance exactly corresponds to that of the expulsion of the *Marranes.* Fulgosi announces its existence in Lombardy as early as 1492. We find it among the Germans in 1493, 1494. John Pomarus says it appeared in Saxony in 1493.† Henry Bunting affirms the same thing for Brunswick and Lunenburgh.‡ According to John Sciphover, it broke out in 1494 in Westphalia, from whence it soon spread to the coasts of the Baltic sea, to Pomerania, and Prussia.§ And as mentioned by Linturius, it manifested itself in 1494, on the borders of the Rhine, in Suabia, Franconia and Bavaria.‖ Now, the expulsion of the Marranes dates from the year 1492. These unfortunate wretches, who left Spain, according to Fabricius, to the number of one hundred and twenty-four thousand families, or of one hundred and seventy thousand, as mentioned by Mariana, lost, according to the same Fabricius¶ and John Nauclerus, thirty thousand families of a most fatal epidemic, which appeared of a peculiar nature. The disease not merely spread to Rome, as mentioned by Infessura, but also infected Naples, according to Zureta and Collenuceio, and even was propagated to the coasts of Barbary. Leo the African says, that the disease, anterior to the landing of the Marranes, was unknown in Africa.** Paul Iovius attributes also the extension of

* Annal. Hirsaugens. T. 11. ad. a. 1496.

† Chronica des Sachsen und Niedersachsen. Wittenberg, 1589.

‡ Neue Vollstaendige Braunschw. und Luneb. Chronice Magdeb. 1620. p. 293.

§ Chronica archicomit. Oldenburg, in Meibomii script. rer. Germ. T. 11. p. 188.

‖ Append. ad. fascicul. tempor. in Pistorii script. rer. Germ. T. 11. p. 106. 108. 110.

¶ Hist. rer. Germ. et Saxon. ad. a. 1492.

** De totius Africæ descriptione libri ix. in 8vo. Figur. 1559. l. 1. 30. p. 56.

the disease to these exiles.* Finally, some passages from Peter Martyr,† Francis de Villaloros, and Peter Pinctor,‡ which, owing to their want of clearness, have been refuted by the partisans to the American origin, seem to indicate that the epidemic already existed in Spain, during the last twenty years of the fifteenth century; consequently before 1490. It cannot at all appear surprising, that such considerable collections of people, whom the avarice of Ferdinand had deprived of all the necessities of life, and consequently thrown into the most disgusting filth, the inseparable attendant on misery, should have spread, wherever they passed, a contagious cutaneous disease, complicated with scorbutic symptoms, which were necessarily produced by the dampness and excessive heats of the weather, that are noticed by all the historians, joined to the total privation of all wholesome aliments. This is the idea which we naturally form of the terrible epidemic of the fifteenth century, when we read the minute description of it given by Joseph Grunbeck, and especially that of Fracastorius, whose rare talents found it easy to spread over a subject, the least proper to inspire him, the charms of the most delicious harmony, and whose poem, justly called divine by Julius Cæsar Scaliger, merited the suffrages of the celebrated secretary to Leo X, the cardinal Bembo, so well known for his almost fanatic enthusiasm to fine latinism.

This epidemic soon gradually diminished, and it appears, according to Guichardin and Ulric de Hutten, that it retained

* Historia sui temporis, l. iv. p. 79. T. 1. Lutet. 1558.

† Epistolæ, 1530. Alcala Epist. 68. The letter of Pinctor to Arius is dated 5th of April, 1488. It was first spoken of by Sanchez—Girtanner declares it false, because syphilis is in it called *morbus Gallicus*, which name was only given to it in 1495, and because Arius, to whom it is addressed, was professor of the Greek tongue at Salamanca, which chair was only instituted in that University, in the year 1508. As to the first objection, we may answer that Peter Martyr calls the disease also *bubas*, that this name was given to it by the Spaniards, before it was called *morbus Gallicus*, and that these latter words are only an addition of the editor, to render the text more intelligible.

‡ Tractatus de Morbo fœdo et occulto his temporibus affligente, cap. iv.

its primary pestilential character for the space of only seven years. Now as at that period the use of linen, which was introduced, the progress of civilization, and the severe measures adopted by the police, caused in some respect leprosy to disappear; and as, on the other hand, the aggravated debauchery of the times multiplied the venereal affections to a wonderful degree; and finally, as the venereal act being the most direct source of the contagion, owing to the intimate contact existing at the moment; it was principally during it, that the marranic affection was transmitted. It was soon believed that this last, together with the venereal symptoms, formed but one and the same disease, which received the name of syphilis,—was supposed afterwards to have insensibly mitigated, and to have degenerated from leprosy. Leoniceno, although combating this error, was however, from the manner he explained the origin of syphilis, foremost in its propagation. In support of his explanation, he undoubtedly had the observation already made, afterwards repeated by Charles William Nose, and at different times verified in the hospitals of Paris, that the atmospherical constitution exercises the greatest influence on venereal affections, and that many of these latter, blennorrhagic discharges for example, are more frequently met with in some seasons, or some years, than in others. This remark is correct—with a little attention indeed, we should, perhaps, find that the affections of the throat, that are generally regarded as consecutive syphilitic symptoms, but depend on a more sympathetic irritation, coincide strikingly with the developement of catarrhal affection of the throat, caused by atmospherical agencies.

The learned Hensler, who has so much contributed to the progress of the history of syphilis, also conjectures that this disease results from a degenerated lepra, or else from a sort of fusion of that malady. This sentiment, somewhat modified, was adopted by the erudite Sprengle, who believed that syphilis had originated from a peculiar combination of lepra, with the venereal affections already known in antiquity. These two writers were supported not only by the fact

already observed, and especially noticed by Sydenham, that the atmospherical constitution can, under certain circumstances, and at certain periods, change to such a degree the character of chronic disease, as to make them assume an aspect truly epidemical ;* but also by the fact, that the first physicians who wrote on the *morbus gallicus*, almost unanimously regard it as the ancient leprosy, merely disguised under a new and peculiar form, and which appeared the more probable, as the period of the first appearance of this epidemic in Europe, is precisely that of the extinction in Europe of the most frightful of all leprous affections, the black lepra or elephantiasis. Indeed, Sebastian Aquilanus endeavours, by all possible means, to detect some resemblance between the syphilis of his time and elephantiasis. Cataneo informs us, however, that Sebastian had never seen this last affection; and of this we ought not to be astonished, for Fracastorius says that physicians, for a long time past, had not a correct idea of the elephantiasis of the ancients, and consequently thought themselves authorised, from feeble comparisons, to apply to the *morbus gallicus*, the descriptions which their predecessors had left of the former disease. But there existed much more analogy between the morbus gallicus and several other varieties of lepra, and especially with that, known under the name of *malum mortuum*, *morpheus* and *saphati;* consequently, Gaspard Torella, and John de Vigo, continued to view the French disease as a species of *morpheus*, Widmann ranged it among the *saphati*, and called it *pustulæ asafaticæ seu formicales :* Sebastian Brandt gave it the epithet of *impetigo pestilentialis*, and by J. Grunbeck and Wendelin Hock, it was compared to the *mentagra.*†—In-

* In this way Hensler refutes the opinion of some writers, lately repeated by Swediaur, that the *pestis inguinaria* of the middle ages was syphilis. He thinks that the plague may have sometimes assumed a character such that buboes appeared in preference in the groin. Now it appears that an epidemical constitution of this nature reigned in Italy about the end of the 15th century, at the same time with the marranic disease ; and that after continuing a few years, it disappeared.

† Syphilis seemed to attack in preference the wealthy, as did the *menta-*

deed, such was the degree of persuasion as to the great affinity existing between these diseases, that Conrad Schellig, the first writer on the syphilis of the fifteenth century, after stating that it is allied to the formica, adds, *de nominibus non est curandum, cùm res ipsa nota sit.* Far from considering it altogether new, he regarded it merely as a disease, spread more widely, in his time, than it had ever done before, and become epidemic, in the same manner that we now see small-pox or dysentery assume that character. To expose all the motives on which were grounded these comparisons, would occupy too much of our time, and may be found amply enumerated in the interesting work of Leoniceno. But what is essential to remark is, that the most inveterate enemies to this opinion, such as Leoniceno, Fracastorius and Cataneo, granted that in some cases there really existed some affinity between these two diseases. Cataneo for example, assures his having seen in two cases, the *morbus gallicus*, degenerate into elephantiasis ; of which conversion, a long time after, Fischer and Kniphof announced having seen one instance in a young man, in whom salivation had failed to cure a venereal disease.* It is therefore not surprising that the opinion of the affinity existing between lepra and syphilis, should have made so many partisans. Peter Maynard maintained that these affections only differed in intensity, and George Villa applies to the morbus gallicus, what others had said of lepra. The same opinion is embraced by John Manard, and finally Paracelsus, in his singular language, gives to syphilis of his times,

gra described by Pliny. It is well known that it spared no one member of the incestuous families of Alexander VI. many of whom even died. For this James de Bethencourt gave it the name of morbus magnatum.

* It was long known, and Aëtius had himself remarked it, that women are less affected with leprosy than men. Now as it was soon found that they are likewise less subject to venereal affections, this was employed as a new argument, in favour of the affinity of the two diseases. This prerogative of women was attributed to the dense and cold nature of the uterus, which necessitates a long continuance of the semen in that viscus to produce infection ; whilst the genitals of men, being of a warmer nature and porous texture, received with facility the germs of the infection.

which he regarded as having degenerated from the ancient, the name of hybrid, arising from the combination of lepra, with the primary morbus gallicus, which he called *cambucca.**

Beckett, therefore, advanced nothing new, when he maintained, with more wit than truth, that syphilis had primarily originated from leprosy. This same opinion, Raymond had not been far from embracing, and Frederick Allamand very singularly approached it, when he maintained that the venereal disease arises from the yaws, and it would seem as if it had been almost universally adopted; for when we read Tournefort, Hasselquist, Cavazzi, Labat, Pouppé Desportes, and a number of others, it is almost impossible to determine whether they, in their accounts, allude to lepra, syphilis, or the yaws.

Others, on the contrary, taking merely into consideration the mitigated ravages of an epidemic, the pestilential character of which could not but decrease after the removal or diminution of its causes, and confounding it also with the venereal affections which oftener than ever presented themselves to their observation, imagined that the venereal disease was susceptible of passing through different periods of increase and decrease, and finally, of disappearing for ever. This idea was first advanced by Fracastorius, and afterwards repeated by Raymond and Schwediaur. Raymond was of opinion that the virus has always existed, though in a lesser degree, in the warm latitudes of Asia, which accords perfectly with the assertion of the authors of the Asiatic Researches, that syphilis was at all times known to the Bramins. But he also believed that the effects produced by this virus had with time degenerated, and that it would itself, one day,

* By *Cambucca*, Paracelsus understood an ulcer of the genitals caught from a woman, (*Georg. Christoph. Detharding, Diss. de Cambuccâ Paracelsi, in 4to Rostochii*, 1756.) From this lepra, for thus he called it, combined with the ordinary lepra, originated syphilis or the *lepra Cambuccina* The *Cambucca* has therefore, according to him, given the venereal taint to lepra, and the lepra cambuccina has caused the common lepra to disappear.

disappear in those countries, when the culture of the soil would dissipate those deleterious effluvia with which the atmosphere is charged. Doctor Schwediaur goes further: he maintains that syphilis has already several times travelled around the globe. "It is possible," he says, "that the virus in thus spreading and multiplying itself, becomes by degrees divided and extenuated, so that it is at length extinguished, and disappears from at least a portion of the globe, probably to reappear after ages or thousands of ages, with redoubled violence in one or more parts of the earth." According to him, all the affections of the genital organs described by the ancients, should be considered as the effects of a venereal virus degenerated, or whose energy, if we may use the expression, has been used and exhausted. This idea, though less consoling than that of Raymond, is however, more ingenious,—though we are of opinion that it presents one of the grossest errors imaginable. As long as man shall live, so long will venereal affections appear, since they are the inevitable consequence of excesses to which he is infallibly led. These diseases will even spread the wider, in proportion to the progress of civilization. They in fact will only be mitigated, when physicians, ceasing to regard them for more than they really are, will for ever renounce a system which leads to a method of cure far more prejudicial than the disease itself. But then the profits of empirics being less, the personal interest of these able explorators of that rich mine, public credulity, will eternally oppose an almost insurmountable barrier to a revolution, towards which we look with impatient expectation.

We here terminate these observations, to which we might have given much more extension, if the intended limits of these chapters had not forced us to restrict ourselves to the most essential points, on which, however, on some future day we intend entering more largely. Our object has been to prove

1st. That there exists no venereal disease.

2d. That there exist but local venereal affections that are contagious.*

3d. That these affections were known at all times *among the civilized nations.*†

4th. That they are very different from the epidemic of the fifteenth century.

5th. That they did not, any more than this latter, originate in America.

6th. Finally, that the actual system of the venereal disease results from the changes that have been introduced into the medical doctrines, both by the modification supervening in manners and circumstances, and the progress made in anatomy.

* We merely pretend that these affections do not depend on an only and specific cause; for it would be absurd to deny that with time they can produce manifest derangements in the animal economy. In proof of the acrimonious nature of venereal virus, physicians cite those frightful ulcers, which supervening on ill treated buboes, destroy the teguments of the abdomen, and even the aponeurosis and muscles. But the same thing is observed in large atonic ulcers of the lower extremities, that are not at all attributed to a specific virus, and which, like those following buboes, are too frequently the cause of marasmus and death.

† The defenders of the new origin of syphilis are principally supported by the fact, that this disease is not met with among savage tribes. It would really appear strange that this malady should exist among men, who, far from carrying the pleasures of love to excess, only make use of them from a natural instinct to satisfy their pressing desires. License results from wealth and idleness; but the savage, deprived of all, has no more time than necessary to procure for himself a precarious and uncertain nourishment; and if after this he has any moments of leisure, he makes use of them to taste the sweets of repose, towards which in all climates he seems particularly inclined. Did brutes carry those pleasures as we do, to excess, like us would they certainly be affected with venereal affections. We find, in the *Magazine of Hamburg*, the history of a dog affected with a contagious discharge from the urethra. Now we well know that the dog is a very lascivious animal, and the one alluded to manifested a most extraordinary ardor. If among the savages we discover no traces of venereal affections, we on the contrary find them among nations just emerging from the infancy of civilization. The learned Foster, and Captain King, observed them among the Otaheiteans and other inhabitants of the South Sea Islands.

We are perfectly aware that these conclusions will meet with warm opposition. Voltaire has said, "It is believed the French are fond of novelties; but this is true only with respect to cooking and fashions; as for truths, they are always proscribed among us when new, and are only well received when old."

We have not aimed at innovation, but have endeavoured to conform ourselves to that fine maxim of Tacitus: *ad utilitatem vitæ omnia inventa factaque nostra dirigenda sunt.* Supported then by the purity of our intentions, we will say with Sinapius: *Si quis curiosorum quærat cur hanc dissertationem de morbo gallico, uti plerisque videbitur absurdam et parodoxam, concinnarevim, judicet quis quid velit: quæ enim aliis absurda, eadem mihi vera videntur.*

ART. IV. *On the Effects of Deleterious Effluvia upon the Animal Economy. Read before the Medical Society of Philadelphia on the 16th of February,* 1822. *By* J. C. ROUSSEAU, M. D.

ABOUT the year 1814, with a view to attract general attention to the modus operandi of miasmata, upon the animal economy, a subject that I not only considered of the utmost importance, but on which a number of observations had induced me to entertain an opinion very different from that generally received, I instituted a series of experiments upon several of the vital organs, and handed their details with the results to my friend Professor Chapman, with a request to have them published in the Eclectic Repertory.

This request, probably from the multiplicity of his engagements at that time, was forgotten*—and those experiments, though referred to by several who had a knowledge

* The paper in question was at the time presented to the editors of that journal, who probably declined publishing it, as their work was made up of selections only.—EDITOR.

of them, having never been given to the world, and their author being yet unknown, I had, long ago, formed the resolution of embracing the first opportunity of presenting them to your consideration, with a sincere desire that they might become a source of investigation, and of future improvement in our science.

Being satisfied that all the phenomena of organization are the result of the agency of matter upon matter, my attention was particularly and essentially directed towards those organs, which by their situations, connections and functions, are the most exposed to the influence of external agents: and though well aware of the subtle nature of all kinds of emanations—but three inlets, namely the skin, the sense of smell and the mouth, seemed to me capable of affording a passage to the internal organs, so as to favour the admixture of miasmata, as it is believed, with the general mass.

From my former experiments on cuticular absorption, published in the year 1800, and a multiplicity of others, instituted since and attended with the same result, it appeared so evident, that no injury could be apprehended through the medium of the skin, that nothing new was to be anticipated to compensate the loss of time devoted to the same subject. The sense of smell and the mouth became then the only objects of my investigation.

The close connection of these organs, and their vicarious functions in assisting the two principal motors of life, the lungs and the stomach, required, I was aware, the strictest attention, as well as the most scrupulous discrimination, on account of the different results which might be obtained from the admission of various substances through either of their cavities.

No better course, in my opinion, could be adopted to arrive at a satisfactory conclusion, than a series of experiments, conducted and diversified in such a manner as to ascertain positively the organ through which the baneful influence was imparted—for I was, I must confess, decidedly opposed to admit that miasmata are carried through

the general circulation, nothing indicating their presence in the fluids, and the stomach appearing, generally, of all the organs, the most affected, even when it was entirely out of the reach of the deleterious agent.

The feeble degree of susceptibility of this as well as of the pulmonary organs, for a number of stimuli which are known to affect the sense of smell in the most powerful manner, was, however, too clearly established by well supported facts, to induce me to look to the stomach or to the lungs for the source of the derangement. But the evident intimate connection between all the parts composing the system of the mucous membranes—the want of appetite, and the total loss of taste for aliments experienced during the existence of a coryza, strongly indicated that the action produced on the Snyderian membrane might be the primary cause of the ostensible effect in the stomach.

I had for a long time suspected, and I soon convinced myself, that when the nostrils are so completely closed as to preclude their participation in the acts of breathing, eating and drinking, the palate and the whole cavity of the mouth and fauces, far from being, as it was generally believed, the residence of taste, are extremely blunt organs, and possessed of no other faculties than that of being sensible to the mechanical action resulting from the mastication of food: that the most nauseous, the most inodorous, and the most aromatic substances, are all alike, upon the unassisted palate. It seemed to me no less clear, that the sentient power of the Snyderian membrane is in no manner different from that of the other organs, a friction of the emanated particles upon the nervous papillæ in their passage with the atmospheric air, being indispensable to produce an excitement—the sensation being always proportionate to the rapidity of the column of the air loaded with the odoriferous particles.

Yet it was not evident that the olfactory organ was exclusively affected, and that it could transmit a deleterious influence to remote parts, when no sensible degree of excitement was manifest in it—though such a conclusion

might have been drawn from analogy with the highest degree of plausibility. Thus the morbid effect proceeding from the sight of certain objects—from discordant sounds—from the act of tickling and many other disagreeable sensations—the swooning, from the simple prick of a pin, or the mere anticipation of pain—the general relaxation, and, not unfrequently, the death caused by fear, are too generally known not to warrant the conjecture, that certain stimuli applied to the Snyderian membrane might convey analogous sensations to some of the internal organs.

This, moreover, appeared fully demonstrated in the act of sneezing, from the simple titillation of the internal membrane of the nostrils, or from the application of a number of dusty substances to its surface, when no emanations could be received in the bronchia, nor carried to the stomach.

A vast field was offered here for ingenious theories. But from these much light could not be anticipated on a subject of this nature. The result of well conducted experiments claimed at once the preference, and was deemed the most appropriate to this inquiry.

A series of them was accordingly instituted—and to obviate as much as possible error and fallacy, the subjects upon which they were tried were selected among those who could be least suspected of entertaining any preconception, or of being under any impression calculated to favour imaginary feelings, and otherwise frustrate the purpose of the investigation. The substances used were chosen among the most active of those that seldom or ever fail to produce a powerful effect upon the animal organs

The utmost impartiality, and a scrupulous attention were observed during the whole course; and neither place, time, nor individual caused any difference in the results, or gave the least reason to suspect the slightest inaccuracy.

Sensible, however, that experiments conducted with all the care and sagacity, in other hands, and under particular circumstances, are not unfrequently attended with different results, I have entertained the opinion that no better

method could be adapted to expose errors, in case some have crept in, and shelter myself from unmerited imputation, than to relate minutely the mode of proceeding that has been pursued—declaring at the same time, that I have not relied solely upon my own judgment, but received the assistance of my friend Dr. S. B. Smith, and of several other gentlemen fully competent to determine correctly. I have also thought proper to confine myself to the recital of a small number, no deviation of effect having been observed in the multiplicity of my experiments that seemed to deserve particular notice.

Experiment 1*st*.—A professional gentleman with whom I had several arguments on the present subject, unwilling to concede that the sensual pleasure derived from eating and drinking resides entirely and exclusively in the olfactory organ, consented to trust his conviction to experiments. He was, for this purpose, blindfolded, and his nostrils were rendered impervious by compressing them between the fingers. In this state a small lump of camphor was put in his mouth, and masticated for some time: unable to determine the nature of the substance acting then solely upon his tongue and palate, he declared it to be a piece of crumb of bread with pepper on it.

Experiment 2*d*.—A medical gentleman still more averse than the former to relinquish his fixed opinion, volunteered himself to perform a similar experiment.

Having previously been blindfolded and with his nostrils well secured, a small portion of assafœtida was substituted for the camphor used in the preceding experiment, and put in his mouth. He turned and turned it again between his tongue and palate, and exultingly exclaimed that it was camphor.

His mortification cannot be described on finding himself so severely disappointed when he was restored to the exercise of the senses he had momentarily been deprived of.

Several persons of the company tried a number of experiments with different wines, cheese, and many other

things, and were convinced of the correctness of the observation.

Experiment 3d.—The emanations of several essential oils were inhaled through the mouth exclusively, without communicating sensations capable of giving a knowledge of their particular quality. But as soon as the breath was returned from the lungs, through the open nostrils, the discrimination was made.

Does not this last experiment show, that the lungs possess only a feeble power of decomposing emanations remaining for some time in the bronchiæ?

Experiment 4th. Much having been related of the active effects of tobacco applied to the surface of the body, I was induced to try whether the nausea and vomiting produced by such an application, are owing to an absorption from the skin, or to the action of the ascending fumes upon the pituitary membranes, or to a mixture of the volatile particles with the blood, through the medium of the pulmonary organs.

I resolved to make a few experiments to ascertain the truth, and at the same time, to enable me to assign the real cause with some degree of certainty. A stout Irishman, unapprised of the intention of this experiment, was hired for the purpose. His nose was made impervious, with lint, secured by adhesive plasters. A bath of a strong decoction of tobacco having been previously prepared, he was put, and remained in it, up to the navel, for one hour and a half, without evincing the least symptoms of nausea, or any other uneasiness—whereas, the bystanders, called to witness this experiment, laboured under such a degree of nausea as to be put to the necessity of leaving the room, and some of them suffered from severe vomiting.

Experiment 5th. A child upwards of seven years old, was plunged up to the neck, and remained in a bath of the same kind for two hours, his nose being secured, and suffered so little inconvenience, that he ate cakes during all the time.

Experiment 6th.—A lady of a delicate constitution, ex-

tremely prejudiced against the smell of tobacco, having repeatedly been sickened by the breath of gentlemen chewing this vegetable, struck by the relation of such singular facts, was induced to try some experiments. She first convinced herself that when her nose was closed, there was nothing nauseous. A quantity of tobacco leaves having been put in a large pan, with a gallon of water, over a chafing dish, and suffered to boil for some time, she breathed the fumes through her mouth, holding her nose with her fingers, for half an hour, without experiencing the least nausea, or any other disagreeable sensation.

Experiment 7th.—A child three years of age, labouring under a violent coryza, which placed him under the necessity of breathing exclusively through his mouth, had a poultice of tobacco leaves applied upon his belly. It was kept all night without producing any apparent derangement.

Experiment 8th.—A labouring man, not remarkably abstemious of spirituous liquors, was persuaded to try the next experiment. A quantity of good spirit was poured into a large copper pan, and kept warm, by means of a chafing dish placed under it. His nose having previously been stopped with lint and adhesive plasters, he was desired to stand over the pan, and breath the rising steam through his mouth. In this situation he remained above one hour, much deceived in the treat he had anticipated, having experienced no other sensation than a smarting of the throat. I tried, myself, the same experiment, breathing for one hour and an half the fumes of the liquor, and found the same result.

Experiment 9th.—The next day, with a view of making a counter experiment, to ascertain the truth of intoxication having been effected, as it is reported, by exposure to spirituous emanations, the same individual was desired to repeat a similar process, with open nostrils. He had scarcely remained half an hour over the liquor, when he begged to be excused, declaring that he felt so giddy that he did not think he could stand, and actually staggered in going to a chair.

It seems to me quite unnecessary to consume time in relating

a number of other experiments, all tending to corroborate the same facts, and feeling fully satisfied, that every reasonable and unprejudiced mind will join me in the conclusion, that the symptoms generally attributed to an absorption of deleterious effluvia, or to their admission into the stomach, after mixing with the saliva in their passage through the mouth, are simply and exclusively the result of their action upon the membrane of the nostrils and the olfactory nerves—and that in those cases the stomach is only affected secondarily by sympathy, or agreeably to a law of the animal economy, which we, although unable to explain, are compelled to admit, from the evidence of our senses.

From the above results, presuming that they hold good in all cases, might we not infer, that the Snyderian membrane is the vulnerable point, by means of which the baneful influence of contagion is received and transmitted to various parts of the animal system? Miasmata must essentially exist under a form susceptible of being carried by the atmosphere, the common vehicle of subtle matter, and therefore unavoidably and constantly come in contact with the internal surface of the nostrils. The stomach, on the contrary, to which they could only be conveyed by a mixture with the saliva, or by absorption through the lungs, can scarcely experience any such influence. In numerous instances too, the effect is so sudden, that it cannot, with plausibility, be referred to any of these latter sources,as we shall have presently occasion to demonstrate.

If I have taken a correct view of the subject, and satisfactorily ascertained the organ primarily affected, the use of scented bottles, volatile salts, aromatic vinegars, and a number of other active substances, intended, and calculated to exert their influence upon the membranes of the nose, cannot appear altogether destitute of salutary consequences, especially if we advert to the regular train of our sensations; for it will then become evident, that the application of a new stimulus to an organ seldom fails to disturb or subvert its regular functions, from which we may presume that the sense of smell, under the constant influ-

ence of the repeated and compound action of various stimuli, eventually breaking the natural link of associations, receives impressions extremely different, and may thus be partially or totally disqualified to transmit the effects of the baneful emanations—supposing that the mixture with the volatile drug was insufficient to neutralize them.

This is, I think, fully exemplified in the act of taking snuff, where the mechanical action of the powder being stronger than that of the effluvia or aroma of the plant, upon the membrane of the nostrils, it becomes preponderant, by which a nausea is prevented, though the diaphragm, the abdominal muscles, and, we may say with exactness, the whole frame is roused to a violent momentaneous convulsion. The same phenomenon would take place, if at the time the open nostrils are exposed to a nauseous emanation, they were titilated with a feather, or acted upon by a stronger stimulus than that produced by the emanation.

This law, as far as I know, will be found to hold good in the greater number of our sensations, which generally appear to be in an inverse ratio of the intensity of the stimulus, and it is also not only corroborated by daily experience, but supported by the following facts.

The sight of a few drops of blood, not unfrequently produces disordered sensations, that the carnage of battles had never excited.

The slow motion of a large ship, rolling upon a long sea, sooner disorders the head and stomach, than the quick tossings of a small craft under a gale of wind.

The murmur of a gentle stream invites to sleep, while the roaring of a torrent drives it afar.

How many different sensations are produced by various shades of light? To what extreme is carried the power of harmony? And what disordered associations are not excited by discordant or shrill sounds?

An atmosphere impregnated with the sweet aroma of flowers, often brings on a state of debility, that may be relieved by the pungency of their essential oils, applied to the nostrils.

The unpleasant sensation of an itching, is relieved by the stronger action of scratching.

The same stimulus, by an increase of excitement, will raise from the pleasant smile to the highest degree of passion.

The sudden application of a strong degree of cold imparts strength to a body debilitated and benumbed by exposure to a low temperature, or a milder degree of cold.

The heat of a strong fire produces a similar effect, when we find ourselves debilitated and exhausted by the temperature of a hot summer day.

Tepid water will favour vomiting, whereas cold or warm water will put a stop to it.

Many persons cannot bear to have the soles of their feet tickled, who can walk with firmness upon a rough gravel. Indeed all our sensations are so diversified or counteracted by one another, that their effects are without bounds.

On the other hand, experience and observation teach us, that a number of the most active medicines are greatly altered, and often lose their power by mixture. Opium and tartar emetic mixed together, afford remarkable instances of this nature.

The noxious effects of jalap, of Peruvian bark, of assafœtida, and many other drugs, disappear by an union with aromatics.

Castor oil, so extremely nauseous to many, in the state of emulsion becomes rather agreeable than otherwise.

Can we not, on this ground, reasonably suppose, that contagious miasmata may be so modified by mixture with volatile substances as to become harmless, or that the organ which receives their action may, by various means, be protected against their influence?

It will, I think, at no remote period appear surprising that the sense of smell has so long been and is still thought of so little importance, for the greater portion of the phenomena produced in the animal economy through this organ are not only unknown, but those which have been ob-

served had not, previous to my experiments, excited the least suspicion of their cause.

Had the sensibility of this organ attracted the earlier attention of medical men, it would have, it may be presumed, opened a vast field for observation. A number of affections, too generally attributed to the effects of cold, might have been found to proceed from miasmata floating in the atmosphere, and the rapidity with which a number of epidemics have spread through large extents of territory was such as ought to have indicated it.

In the brute creation, the sense of smell appears the *sine qua non* of animal existence. Is it impaired? the animal forgets or becomes indifferent to its food: It wanders unable to regain its habitation—the sexual desire itself loses its empire over it.

Every fact, every argument tends to prove that the organ of smell is the accessible and vulnerable point. It is the first to receive, and must, inevitably, be the first to suffer from the impression of deleterious effluvia. Why should we suppose, for we have no proof, that the stomach is the receptacle of contagion? and if it were, is it not possessed of a digesting power by which it exerts not only a particular, but an unparalleled control over all substances presented to its action? Does it not receive with impunity the venom of the viper, the matter of the small pox, the venereal virus? Besides, the extreme susceptibility of the olfactory organ is as well demonstrated by common experience, as is the tardiness of the stomach to answer to the stimuli presented to it. One drop of water of ammonia, thrown into the cavity of the nose, produces a greater action than fifty drops in the stomach.

It may, however, be contended, that the deleterious consequences resulting from an exposure to contagious miasmata, are seldom evidenced upon the organ of smell—that the enemy comes clandestinely and without warning—that this organ seldom manifests the least mark of morbid derangement. Let these objections be admitted, will not the same arguments hold good against the stomach? Is the warning

better at this quarter? Does the stomach indicate, at first, the presence of an emetic that is to throw it into the most violent convulsions? Does it inform us of the presence of opium, or of many vegetable poisons, when they are to bring death in a short time? Can we doubt of the existence produced on the Snyderian membrane by the application of errhines, though the diaphragm and abdominal muscles evidence the effect?

Does the ear bear marks of the rapture excited by the power of harmony, of eloquence, or of the sounds of the voice of a beloved object? Does the terror imparted by an awful scene leave any impression upon the eyes?

Does the sight of a sumptuously dressed table, leave on any of the external organs traces in evidence of the anticipated pleasure that it creates?

And yet should the olfactory organ be censured for this unofficiousness? or should we not attribute this unnatural incapacity to the effect of our social institutions, of our habitations, of the aliments we eat and drink, of the clothes we wear, and of multifarious habits, all affecting and blunting, more or less, its natural instinct. The contrary is too well proved by the well known sagacity of this organ among the savages, to doubt the effect of these causes. But are the first symptoms of disease sooner marked by the stomach? certainly not.

A degree of uneasiness is the first precursor—a general lassitude soon follows: loathing of food, heaviness at the præcordia, despondency, derangement in the arterial system, and a multiplicity of other symptoms precede the sickness at the stomach.

The same train of sensations is likewise observable in those instances where the organ of smell is exclusively affected. Why, then, theorise upon suppositions, against the evidence of palpable facts?

The stomach it will be argued, and it is true, in the examinations POST MORTEM, is the principal, and frequently the sole organ that betrays marks of disease—and so far this argument appears of some weight: but, in such examinations,

has the membrane of the nose ever been of any consideration, or engaged the critical attention of the anatomist? Does our knowledge of the texture and organization of these parts warrant a competent decision in such cases? I presume not.

If the difficulty of answering objections, that would have the same bearing upon every case, be for a moment laid aside, and we advert to the *modus operandi* of various stimuli, we shall find that the most serious affections have been brought upon several of the internal viscera, entirely out of the reach of external agents, even when death was the consequence, by a primary action produced upon an organ, showing, in the event, no symptom of disease.

In those violent spasms, for instance, so distinctly marked, of the jaws, and the muscular system, of which scarcely in any instance can the origin be assigned, do we think of looking for the cause of the disease in the part the most conspicuously affected? Are the loss of the beard after castration, and all the other changes that take place in animals by this operation, looked for in the chin, or in any of the parts, most altered and injured?

The innumerable sympathies, or associations, denominate them as you please, exhibited by the phenomena of organized bodies, give us, indeed, the strongest reason to believe that the disorganization of the stomach, is not the primary cause of death, but that this viscus, on account of its importance, and the share of action it holds in the animal machine, exposes more evidently to our senses, the traces of the morbid influence imparted to the whole frame. If I may be permitted to indulge in speculations, facts are not wanting to prove, as I have pledged myself to do, that the greatest part of the morbid affections of the brain, are too sudden to be referred to any other cause, than impressions received through the medium of the Snyderian membrane.

The immediate sense of giddiness, experienced from eructations passing through the nose, after drinking malt liquors or some of the mineral waters—in eating mustard,

or horse radish—in snuffing ammonia, affords ample evidence to this purport.

The sudden shock given to the brain by the introduction of a fluid, as pure water, into the cavity of the nose, which I have always believed to be the first cause of suspension or extinction of life in drowning, corroborates the same opinion.

I might go further. Will it be believed, if I say, that inebriation itself, is not the sole effect of the liquor upon the stomach, but is caused more by the fumes rising from this viscus, and passing through the cavity of the nose? The fact is, I have ascertained, beyond doubt, that no intoxication can take place, or, not to be so positive, only in a very feeble degree, when the nostrils are rendered perfectly impervious.

Consulting nature, we shall observe the instinct displayed on many occasions, but more particularly during our former epidemics, by those who came out of their doors to gaze at the *death vehicle* of the numerous victims, falling at every hour of the day?

It was then extremely common to see individuals, holding their noses, with gaping mouths standing on the footways, to witness the awful scenes, thinking themselves thus perfectly secured against the attacks of an imaginary, though terrific enemy.

We often wonder without cause, and overlook the necessity of effects. Organization can no more exist without a constant succession of phenomena, than matter without shape, and whatever way nature might direct her course, the same question would stand—why is it so?

Of the most difficult points—of the most hidden causes, we are the most sanguine as to interpretations. Too often indeed, rather than to appear ignorant, we satisfy our pride with imaginary, fanciful and pleasing theories. All phenomena are the result, and cannot take place without the causation of those that preceded them: and theorising without a competent knowledge of their concatenation, is as uncertain as attempting to build an edifice, without paying

any regard to the relations and proportions of the materials that enter into its structure.

Men of science are seldom content with facts. They must ascend to causes, and to cover their fruitless attempts, are incautiously led, to the great detriment of truth, into romantic and visionary speculations.

The musician who charms our ears, the painter who deceives our eyes, the cook that excites our appetite, without inquiring into causes, is satisfied of making use of known effects to produce these sensations?

It would indeed be fruitless, in the present state of our knowledge, to attempt the solution of every problem, and, after all, is it more surprising, more wonderful to see a sensation transmitted from one to the most distant part of an animal, than to hear at the extremity of a log at any length, the scratch of a pin, made at the other end? or to witness the percussion of the string of one out of several instruments, placed at some distance from one another, cause the vibrations of all the other strings in unison, when all the rest, either above or below this unison, remain unaffected?

With the help of theories, however, no difficulty is ever encountered—and to explain this phenomenon, we only want a Newton in music, as we had one in optics. Let, for instance, the theory of the seven colours, in which every one firmly believes, be applied to the seven sounds of music, and the fluid in which we live be composed of seven different kinds of vibrations, as the light has been of seven rays, to answer to the seven notes of the musical scale, the mystery is explained at once.

Such a theory, indeed, could be supported with more plausibility than many we have embraced and cherished with the most faithful attachment. It is to be regretted, and it is too true, that as long as theories do not rest upon a basis of immutable facts, the fancy and versatility of human genius will continue to stamp them with that uncertainty with which, immemorially, they have been characterised.

It is nevertheless sufficiently evident that many parts of the animal frame sympathise together, or vibrate in unison, and from whatever cause this may proceed, the result is the same.

There exist as natural and indispensable associations between the component parts of organization, as between the various colours, and the multiplicity of sounds: the different reflexion of light, the repeated vibrations of sonorous bodies, the infinite combinations of matter, and the sympathies between the vital organs, form a link of phenomena, whose rapid succession leaves scarcely time to discriminate between causes and effects, for no sooner an effect is produced than it becomes a cause in the natural order. Hence the diversity of effects from, apparently, the same cause—the failure of repeated attempts—the uncertainty of our remedies—the unbounded variety of diseases, and the still greater difference in the symptoms which characterize them. One link of the chain broken or altered, the same phenomena cannot be expected. "*Consensus unus consensus omnium.*"

In the catenation of effects must we then, look for the cause of every thing, as well as of the harmony preserved under the unlimited mutations of the elements.

We have much pleasure in announcing, that Dr. Coxe, Professor of Materia Medica in the University of Pennsylvania, has in considerable forwardness a translation of the work, the title of which is annexed.* Of the several histories of medicine extant, more than one of which have great merit, that of Sprengel is confessedly the best. Combining accuracy of reference, with precision of style, and the pro-

* The history of medicine, from its origin to the nineteenth century, by Kurt Sprengel; translated from the second German edition into French by A. J. L. Jourdan, Knight of the order of La Réunion; and revised by E. F M. Bosquillon, D. R. of the Faculty of Medicine of Paris, honorary Censor, &c. &c. Paris, 1815, in 7 vols. 8vo.

foundest erudition, it may be advantageously consulted by every medical man who is curious to trace the different branches of his science, from their seminal beginnings, through the various stages of their progress, to their present advanced and maturer state. By such a course of reading much important information cannot fail to be acquired, and especially in relation to the theories and modes of practice of our predecessors, not a few of which have been too hastily thrown aside from the caprice of fashion, or entirely subverted by those greater revolutions to which the science has been exposed. The ensuing article, which forms the introductory chapter of the work, is fully explanatory of the admirable plan on which the history is written, and will serve, at the same time, as a specimen of the translator's style and manner. EDITOR.

ART. V. *Introduction to a History of Medicine.* By KURT SPRENGEL, M. D. Translated from the French, by JOHN R. COXE, M. D. &c.

THE history of medicine embraces all the changes which at different periods this science has undergone. It does not therefore confine itself to retrace the life of celebrated physicians, nor to enumerate and criticize the works which have appeared on the healing art in general, or on each of its branches in particular. Hence the necessity, too often forgotten, of establishing a distinction between the history properly so called, and the literature of medicine. The first examines in a more particular manner the systems which have successively reigned, the methods on which the treatment of disease has been founded, and the revolutions which both theory and practice have experienced. But since, in order to be well acquainted with the diseased state, it is essential to have likewise accurate notions of health—the history of anatomy and of physiology is connected with that of medicine, within the limits I have assigned it. Thus too, the practitioner being unable to exercise his art without having studied the qualities and properties of the bodies which surround us, and which act

upon us, this history has an intimate connection with those of natural philosophy, chemistry, and natural history. It equally embraces the study of the progress of materia medica and pharmacy, because it is not sufficient to be able to distinguish diseases in order to cure them: it is requisite also to know how to choose, to prepare, and to compound the different medicines in a proper manner. Finally, as all diseases do not resemble one another, the history of medicine divides itself into three great sections, comprehending therapeutics, surgery, and midwifery.

To point out the revolutions which each of these three principal branches of the healing art has undergone, is to write a general history, and such is the end I have in view. But it will be easily perceived, that it is impossible in a work of this nature to enter into all the details relating to the changes of the branches of the art, especially of those which have only an indirect connection with the principal object. This care devolves rather on those authors who write on each of them in particular. In fact, the history of the circulation, and of bloodletting, is much more important for the general history of medicine, than that of the theory of colours, of discoveries in natural philosophy, or of chemical doctrines, which however require to be noticed, since they have exercised a decided influence on the theoretical and practical parts of the healing art.

The history of medicine ought to be written in a chronological order, that is to say, it should expose the series of events the most remarkable in the science, disposed according to the order of time. But since there exists much difference of opinion as to the age of the world, at the birth of Jesus Christ, I have preferred, in order to avoid errors and uncertainties, to speak of the ancient history, by olympiads, or the years which preceded the Christian era. This history can have no real utility unless it points out the different events in connection with each other, develops the causes which gave them birth, and indicates the effects resulting therefrom. It consequently follows, that its chronology ought to be subjected to the same general routine.

It is thus we must pursue the school of Paracelsus down to modern times, although we may be subsequently obliged to retrograde a complete century. Geography is precisely in the same situation: for we cannot trace the history of the medicine of a country, or of a nation, in particular, unless it is in a nation or country altogether independent of that of other people. For example, the medicine of the ancient Egyptians is altogether isolated, and depends in no point on that of the nations which flourished at the same epoch; but it would be absurd to wish to separate the history of the medicine of the Spanish, from that of Italy or France.

The progress of civilization being alone capable of explaining the origin, the progress and the decline of the sciences in general, it is essential if we would render the history of medicine really useful and instructive, to observe attentively the progressive development of the human mind, in order to comprehend the different medical doctrines, to penetrate the issue of even useless attempts after truth, and to rectify the system we ourselves may have adopted.—We should be exposed to the charge of inconsistency, should we think we could attain these ends, by contenting ourselves with developing the causes and the results of opinions and of practical methods; for it is often impossible to discover the secret springs by which the sciences are led towards perfection or decay. We sometimes readily perceive the proximate causes of events, but it belongs to uncommon genius alone to distinguish the remote causes.

The history of the civilization* and of the progress of the human mind, appears to be the true basis of that of the sciences in general, and of medicine in particular. In fact, it can only explain why a scientific revolution should occur in one way rather than in another.—Illumined by its torch, we need not fear wandering in the road of error.—We ap-

* I call *civilization*, the passage of mankind in general, or of a particular nation, from the rude and savage state to that of social life, which supposes a development of the intellectual faculties. See *Adelung's versuch*, &c. i. e. an essay on the civilization of the human race. 8vo. Leipsic, 1782.

preciate correctly the so much boasted medicine of the Egyptians and Chinese, we consider that of the Greeks in that point of view in which it is proper to regard it, we cease, in fine, to regard the appearance of Hippocrates as supernatural, and we see in the salutary reform effected by that great man, only the necessary result of an infinitude of events.

Philosophy in certain respects is the mother of medicine, and the perfection of the one is inseparably connected with that of the other. In combining the history of these two sciences, we learn what were in every age, the extent of knowledge, the predominant opinions, and the genius of the art.—Physicians in fact, have almost always derived their theories from the philosophers. If the rage of demonstration predominated in the schools of the latter, the former followed faithfully the same route, and strove by a display of pompous words and ostentatious expressions, to afford their proofs, an evidence which they did not possess, and which they could never acquire. As soon as philosophers began to introduce a critical scepticism into all human acquirements, the physicians were also the first to reject any principle which was not the result of faithful observation. The more we attend to the history of medicine, the more do we learn to judge of the dominant opinions of each age conformably to the spirit which then reigned in the schools of philosophy. The system of Hoffmann is as evidently the result of the philosophy of Leibnitz, as the chemical system of the last century is that of the dogmas of Descartes. Many even of the essays of the moderns partake of a critical philosophy. But all these medical theories, founded on philosophical principles, by time sink into oblivion, and the dogmatic tone of the medico-philosophers gives no astonishment to the physician who is familiar with the revolutions of his art.

The history of medicine should be written with impartiality; he who undertakes it should embrace no system, should partake of no opinion, but should be eclectic in the full meaning of the term. Yet as it is impossible to close

the access of the heart to truth, it is impossible that the narration shall not partake a little of the disposition of the historian, when he unfolds gross errors, or announces great discoveries and important truths.

To write this history well, he must have read the principal writers of each age, in order to judge of the spirit of the times: but that this reading may be advantageous, he must dismiss every particular opinion, imitate the conduct of a man an entire stranger to the science, but guided by a sound reason, investigate the writings of physicians, identifying himself with them, if we may so say, appreciating the spirit of the age, and seizing on the ideas of each author as if one of his contemporaries.—The historian should have a preference for neither the medicine of the ancients nor the moderns, but should know how to estimate the advantages of each age, and expose their failings with the same impartiality. Formed upon a plan like this, the history of medicine is a real torch of truth, and the fertile source of instruction. If he wishes to render it really useful, he should, at the same time with the revolutions of science, theories and systems, expose to view every accessory circumstance connected with them. Hence he ought to trace the lives of physicians, although only in an incidental manner. Nor is it less necessary for him to indicate the books which contain the different doctrines.

The sources of this history are the works of physicians of every age: but it is necessary to use discretion, to be well assured of the authenticity of books, and to be perfectly acquainted with the language in which they are written. Criticism is therefore an important study, indispensable even to the historian. The medicine of the Arabians, furnishes a striking proof of the errors into which we may be led by merely common knowledge. The authors of that nation are in fact known to most practitioners, only through the medium of the most faithless translations. Hence have arisen the false ideas which are commonly entertained of Arabian medicine. It is equally, from not having subjected the works of Hippocrates to a judicious criticism, that the

origin of anatomy has been made to extend to the epoch of that great man, and that a thousand errors not less absurd have been committed.

A sacred duty of the historian is to draw as much as possible from the sources themselves: otherwise he becomes a mere compiler, whose work may please the curious, but will not satisfy the truly learned. This study of the originals is, to him, what the observation of nature is to the naturalist. How defective would be a system of botany contrived by a man who had studied plants only in books, from the descriptions of others, or from herbals!—Such research is troublesome we must admit—it supposes a vast extent of information scarcely to be expected in all writers. But he, in whom it is wanting, however pure and elegant his diction otherwise, must be content with the simple title of compiler, without aspiring to that of historian.

We find among the historians and even in the poets of antiquity, some scattered facts which may throw light upon the history of medicine, particularly upon that of its origin; yet we should employ those works with the most critical severity.

The true talent of the historian consists in knowing how to combine the facts which he has discovered, so as to form a chain which unites perspicuity with truth. The science of history requires, therefore, the concurrence of all the mental faculties to be enabled to trace out useful truths. It supposes not only the art of assembling together facts more or less known, and of connecting them appropriately; but above all, the talent of drawing with facility exact conclusions, and of arranging them in the most favorable point of view.

The glory to be acquired by cultivating this science, is infinitely superior and preferable to that equivocal and precarious one of the founders of new systems. These innovators appear and disappear like ephemeral meteors: history alone, in after ages, draws their names from oblivion, and with the balance in her hand, pronounces irrevocably on their merit.

The number of authors who have possessed this difficult

art, has always been very small, and certainly more are to be found amongst the Greeks and Romans, than the moderns. Yet if I may be permitted to name some of the latter, I must confess that Machiavel, Hume, Gibbon, John Muller and Spittler, possessed the historic talent in the most eminent degree. Winkleman, in his history of the fine arts, and Tiedemann in that of philosophy, have also given us models as precious as they are inimitable.

Since in history, all reasoning ought to be established on positive facts, it is essential always to settle well those facts, before we hazard the slightest judgment on the progress of events. One of the fashionable follies of the day, is that of pretending to introduce a certain unity in history by simple reasoning, without having examined or admitted the facts, and without having studied the sources from which they may be derived. In fact it is much easier to give a free scope to the imagination, and to construct without trouble such frail edifices, than to take infinite pains to procure that accurate knowledge of facts which should be regarded as the immoveable basis of every historical monument. It is true, the easier the enterprise, the less meritorious is the execution.

The history of the science in conformity to the plan I have traced, is of the highest utility to us. It precludes every unjust judgment, and teaches us, that even in opinions the most discordant and most strange, the impartial historian makes most important discoveries: for frequently the most absurd systems have proved useful in bringing to light some long neglected or long forgotten truths. Partiality is commonly the parent of intolerance. But history teaches us to grant indulgence to those whose opinions differ from ours, and to appreciate what is good in their writings. The historian will always tolerate those who do not think as he does, for he well knows, that, spite of every precaution, the human mind is subject to go astray.—A third advantage of the history of the sciences, and one of the greatest, is, that it teaches us to suspect our own powers, and inspires us with sentiments of modesty. It shows us, that a blind confi-

dence in our own opinions, is almost always a proof of their falsity, or of the weakness of the basis on which they rest. In studying, we are convinced with Pyrrho of Alæa, that the means of examining thoroughly, is to suspend our ipse dixit, and that the wisest plan is to look upon all opinions with indifference, adopting none of them. We may repeat, therefore, to the presuming dogmatists these memorable words of the Sceptics: "The argument you at present allege, was of no importance before the birth of its inventor. Many other persons had great powers of mind before one sprung up to develope its importance. It is possible, therefore, that the reasons which are capable of overturning this argument, still exist, although as yet unknown to us. If we are not actually enabled to reply to your demonstration, it is still not necessary to place much confidence in its value On the contrary, the observations made, ought to repress your confidence, and inspire you with a just doubt as to the proofs which to you appear the most irrefragable."*

We learn also in the history of the sciences to know the errors of others, and to avoid the routes by which we might be conducted to them. When we see how greatly we have shut out the light, by neglecting observation to follow frivolous speculations, we are constrained, if really in search of truth, to interdict every subtilty and reasoning, in order to adhere solely to experience.

A last advantage of this history is to inform and embellish the mind. It procures us an accession of information not to be elsewhere collected, or employed in a way so advantageous.—The dry study of the scholastic and false philosophy of the Talmud, can offer interest only to the real historian, who, from amidst the greatest confusion, knows how to elicit some sparks of truth.

In order to facilitate the study of the history of medicine, we must divide it into certain periods, after the principal epochs drawn from the general history of the world, or from that of the art in particular. The following are those I admit.

* *Sext. Empir. Phyrrhon. Hypotyp. liv.* 1. *c.* 13. *page* 34.

1. Argonautic expedition.	1273—1263 years before J C. according to Petau, Gatterer and Carli.	1. First traces of Greek medicine.
2. Peloponnesian war.	432—404 years before Jesus Christ.	2. Hippocratic medicine.
3. Establishment of the Christian religion.	30 years after Jesus Christ.	3. School of the methodists.
4. Emigration of the hordes of barbarians.	430—530.	4. Decay of science.
5. Crusades.	1096—1230.	5. Arabian medicine in its highest splendor.
6. Reform of Luther.	1517—1530.	6. Re-establishment of Greek medicine, and of anatomy.
7. War of 30 years.	1618—1648.	7. Discovery of the circulation of the reform of Van Helmont.
8. Reign of Frederick the Great.	1740—1786.	8. Haller.

Assuredly many objections may be made to these epochs, and I am myself aware of their insufficiency—but I have always been satisfied with having adopted them. It is proper also to notice, that prior to the first period, we already remark some traces of medicine amongst those ancient people.

I shall at present hazard a rapid and brief view of the entire history of the healing art.

The title of Science, that is to say, the collective assemblage of truths deduced the one from the other, was first given to medicine in the ancient dogmatic school, founded 400 years before Jesus Christ, by the first successors of Hippocrates, Thessalus, Draco and Polybius.

Before this period, the rude information which the as yet slightly civilized Greeks possessed of diseases and the healing art, had been preserved by a happy train of circumstances; amongst others, by the custom of tracing upon

votive tablets the results of the simple observations made, and of the cures effected, in the temples. Philosophy, in spite of its infancy, had already begun to appropriate to itself the theoretic part of medicine, and to treat it, independently of the observations hitherto collected together, in conformity to its predominant opinions.

Hippocrates first taught us to know the true point of view in which it ought to be regarded. He separated it from scholastic philosophy, collected the observations preserved in the temples, and those which he himself had made—fixed the general rules of the science, and particularly acquired immortal glory by his excellent method of treating acute diseases.

His immediate successors were so little animated by his spirit, and deviated so greatly from the route he had pursued, that they quickly yielded to the torrent of their age, and applied to medicine the philosophy of Plato—with which shortly, peripateticism, epicurism and stoicism were amalgamated.

Alexandria was for many ages, the only school in which physicians were formed. It was in that city especially, that the healing art was cultivated conformably to the dogmas of a false philosophy. It became a tissue of vain subtleties described in the jargon of the school, and of frivolous disputes dictated by a spirit of controversy. Alexandria was, it is true, the cradle of anatomy; but this science did not inspire an enthusiasm of long duration. It consisted of objects too material and too true, for minds accustomed to appreciate only inconsistency and frivolity.

Tired with these never ending discussions, unworthy of so sublime an art, and encouraged by the example of the sceptics, the empirics undertook to separate a second time medicine from philosophy, that it might be rendered more useful to mankind. Their school gave birth in the end to that of the methodists, who sought to reconcile dogmatism and empiricism, and to fix the general principles of medicine.

Galen then appeared, the most learned of all the physi-

cians of antiquity. He strove to introduce a rigid dogmatism into medicine, and to give it a scientific aspect, borrowed almost entirely from the peripatetic school. The prodigious number of his writings, their systematic order, and elegance of style, enchained, as by an irresistible charm, the indolent physicians who succeeded him, so that, for many ages his system was regarded as not to be shaken.

At that disastrous epoch, when barbarism bent the whole world beneath its iron yoke, in which science, exiled in the cloisters, was confined to a few obsolete copies of the ancients, and to scholastic commentaries on their works, medicine still threw out a glimmering and languishing ray in the Arabian schools, where it was preserved by the study of the ancients, and by some attempts, insignificant it is true, made with the intention of studying nature herself.

At length, in the fifteenth century, a flourishing commerce, a deep study of antiquity, and the cultivation of the fine arts, re-illuminated Italy. Slowly they succeeded in attaining the spirit of the writings of Hippocrates, and insensibly they returned to the observation of nature, both in the healthy and diseased state. Anatomy was cultivated with the most ardent zeal, and the study of diseases would assuredly have carried medicine to the highest perfection, had not the spirit of reformation, generally extended in the sixteenth century, produced the system of Paracelsus, which overturned all the science, which substituted for the elementary qualities of Galen, the chemical elements like so many demons, and which reproduced all the theosophic and theurgic absurdities of the cabalas.

Medicine was freed from these shackles in the seventeenth century, by Van Helmont, and still more by Silvius. Then an extreme importance was attached to a mixture of the humours.—The precious discovery of the circulation of the blood by Hervey, gave the last blow to the system of Galen, and crumbled it to atoms. But this discovery and the philosophy of Descartes gave birth to the medico-mathematical system, which, in spite of the efforts of the

Newtonians, was quickly abandoned in consequence of the immense difficulties which surrounded it.

Sydenham however, guided by the philosophy of Bacon, sought to restore the ancient empirical school, which many circumstances, such as the introduction of new remedies, especially bark, the popularity of philosophy, the development of the mind, and of good taste, and lastly the intimate conviction of the importance of observation, contributed to spread, at the same time that they gave assurance of a long continuance during the course of the eighteenth century.

Stahl and Hoffmann had founded at the close of the preceding age, a modern dogmatic school. The psycological system of the former, rested on the mystic notions of the time; and the nervous pathology of Hoffmann on the doctrine of the monads of Leibnitz, all the modern dynamic systems, even that of Brown, are merely modifications of this last mentioned one: they alone predominated at the close of the eighteenth century. Still, however, the empiric school of Sydenham had many followers, and the chemical sect had likewise retained some zealous partizans.

ART. VI. *On Phrenology, or the Study of the Intellectual and Moral Nature of Man.* By JOHN BELL, M. D. Read before "The Central Phrenological Society, established at Philadelphia," at its meetings on the 4th and 18th of March, 1822.

I.

"THAT implicit credulity is a mark of a feeble mind will not be disputed, though it may not, perhaps, be as generally acknowledged, that the case is the same with unlimited scepticism.

"In the midst of these contrary impulses of fashionable and of vulgar prejudice, he alone evinces the superiority and the strength of his mind, who is able to disentangle truth from error—and to oppose the clear conclusions of his own

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THE FACULTIES ARE DIVIDED INTO

1. ORDER. FEELINGS.

1. *Genus. Propensities.* Having the organs of

NAMES OF THE ORGANS.	SITUATION OF THE ORGANS.	REMARKS.
1. AMATIVENESS, or physical love.	The cerebellum. At the base of the back part of the head, immediately above the middle of the neck.	Generally most developed in man and male animals; and not considerable till puberty.
2. PHILOPROGENITIVENESS, or love of offspring.	The posterior part of the head, immediately above the former organ.	Most remarkable in women, and the females of other animals.
3. INHABITIVENESS—in animals the disposition to determine the place of dwelling—in man, love of country.	Probably seated above the centre of the preceding.	This is most conspicuous in certain animals that are fond of elevated situations.
4. ADHESIVENESS, or disposition to form attachments.	Outward and upward on each side of the second organ.	Society results from this disposition, which is found in some of the lower animals. In man it disposes to friendship, and it is generally large in women.
5. COMBATIVENESS, or disposition to quarrel and fight.	About the posterior lower angle of the parietal bone; or, in adults, about an inch and a half behind the ear.	Conspicuous in the courageous animals. In man it produces courage, and the tendency to attack.
6. DESTRUCTIVENESS, or disposition to destroy.	On the side of the head, immediately above, and somewhat around the ear.	Very discernible in the carnivorous animals; also found in the human species. When this and the preceding organ are large, they give the tendency to rage and ferocity.
7. CONSTRUCTIVENESS, or disposition to build, &c.	At the temples, above the cheek bones; when large, it gives a kind of square appearance to that part of the face.	Remarkable in those animals which build, and in men noted for mechanical invention.
8. ACQUISITIVENESS, or disposition to obtain or acquire.	At the upper part of the temples, upward and backward a little from the preceding organ.	In excess, this leads to avarice; and, when conscientiousness is small, to theft, dishonesty, &c.
9. SECRETIVENESS, or disposition to conceal.	In the middle of the side of the head, immediately above destructiveness.	Cunning, dissimulation, hypocrisy, &c. are its results, when abused or improperly directed. Legitimately employed, it enables us to suppress improper feelings.

2. *Genus. Sentiments.* Having the organs of

10. SELF-ESTEEM, or self-love.	The middle of the upper posterior part of the head, or top of the back part.	When not checked, it gives rise to pride, disdain, overweening conceit of one's self. Most noted in men. When regulated by conscientiousness, it gives dignity and elevation to the character.
11. LOVE OF APPROBATION, or desire of applause.	On each side of the preceding; or the posterior upper and lateral part of the head.	Ambition and vanity are its occasional products. Most remarkable in women. Well directed, it gives the love of fame, glory, and distinction.
12. CAUTIOUSNESS, or circumspection.	The upper posterior part of the sides of the head, or upwards and backwards, from No. 9.	Timid animals and very cautious men have it in a great degree. It produces fear when large.
13. BENEVOLENCE, or kind affection.	The superior middle part of the forehead, or top of the fore part of the head.	The meek and peaceable animals have it much developed. In man it gives universal charity or philanthropy.
14. VENERATION, or tendency to adore.	The middle of the upper part of the head, a little before the crown.	Conceived to be proper to man, and to dispose to religion; as also to venerate any object to which it is directed.
15. HOPE, or inclination to expect and believe.	On each side of veneration.	In religion this sentiment produces faith; its excess occasions credulity. It paints the future fair and smiling.
16. IDEALITY, or poetic and enthusiastic tendency.	A little above the temples, and immediately above No. 8, but extending farther back.	The opposite of cautiousness. Fancy, enthusiasm, poetical feeling, are referable to it.
17. CONSCIENTIOUSNESS, or sense of justice and duty.	On each side of the following organ.	None of the lower animals are thought to possess this sentiment. It is the moral sense of the metaphysicians.
18. FIRMNESS, or resoluteness of character, &c.	The top of the head, a little in front of self-esteem.	This serves to maintain the activity of the other faculties. It gives determination and perseverance; but in excess leads to obstinacy and infatuation.

2. ORDER. INTELLECT.

1. *Genus. Knowing Faculties.* Having the organs of

19. INDIVIDUALITY, or faculty of knowing external objects.	The upper portion is supposed to give the fondness for history, events, &c. and is called by Spurzheim PHENOMENA.—The lower portion to give a disposition to attend to matters of fact, and is by him called INDIVIDUALITY.	These contribute to universal but superficial knowledge, and are early developed. They are conversant with facts, but do not trace their relations.
20. FORM, the power of considering forms.	In the internal angle of the orbit of the eye.	Form is imagined to be the first quality of bodies that is considered by the intellect.
21. SIZE, faculty of contemplating size.	Supposed to be near the preceding.	Size the next quality considered.
22. WEIGHT AND MOMENTA, &c.; faculty by which ideas of weight, &c. are acquired.	Conjectural; but supposed as above.	Consistency, density, and some other qualities, are supposed to be ascertained by this faculty.
23. COLOURING, faculty of perceiving the harmony and relation of colours.	The middle of the arch of the eyebrows.	This is not in proportion to the sense of sight. Some persons see figure and distance correctly, but cannot perceive minute shades of colour, and *vice versa*.
24. LOCALITY, faculty of contemplating places, situation, space, &c.	From above the inner angles of the eyes, towards the middle line of the forehead, or towards the lower sides of organ 19.	Gives rise to a propensity to travel, takes cognizance of space and localities, and is necessary to a landscape painter.
25. ORDER, faculty of conceiving order, method, &c.	Betwixt the organs of colouring and number. Probable.	The notion of regularity is dependent on this faculty.
26. TIME, faculty of attending to the succession of events; duration.	Uncertain; but supposed to be outward and upward from 21 and 24.	Implies a succession of events. Supposed to give perception of time in music; and the feeling of duration.
27. NUMBER, faculty of calculating, &c.	About the external angle of the orbit of the eye.	Whatever concerns unity and plurality belongs to this faculty.
28. TUNE, faculty of perceiving melody in sounds; musical genius depends on it.	The lateral parts, or external corners of the forehead.	Much developed in singing birds and in great musicians.
29. LANGUAGE, faculty of acquiring and using arbitrary signs.	About the middle of the orbit, so as when considerable to give prominence to the eyes.	Not confined to oral discourse, but applies to all arbitrary signs whatever.

2. *Gen. Reflect. Fac.* Having the organs of

30. COMPARISON, faculty of finding resemblances, using examples, &c.	The middle of the superior part of the forehead, between 19 and 13.	Does not determine the kinds of comparison. Gives a fondness for analogies, and an acuteness of discrimination.
31. CAUSALITY, faculty of examining causes and relations; metaphysical genius.	The superior part of the forehead, on each side of 30.	The general relation of cause and effect is its object. This and the preceding are essential requisites in a philosophic understanding.
32. WIT, faculty of the ludicrous in general; gaiety.	The upper and outward parts of the forehead, on the side of 31.	Gives the perception of the ludicrous.
33. IMITATION, faculty of copying or mimicking the actions, manners, &c. of others.	The superior part of the forehead, on the sides of the organ of benevolence.	Very discernible in children, actors, mimics, &c.

ADDENDA—WONDER, or feeling of the marvellous. Betwixt ideality and imitation. (Probable.) Gives the feeling of surprise, and of pleasure from novelty. In excess, produces astonishment and a tendency to the marvellous.

unbiassed faculties, to the united clamours of superstition and of false philosophy."*

Such is the language of one of the first philosophers of the day, and such ought to be responded by every man, who prefers the mild light of truth to the glare of fashionable error. Much as we boast of our enlightened age, and of our freedom from the thraldom of prejudice, which pressed so heavily on our ancestors, still are there but too many content to float smoothly along the current of popular prejudice, and while courting every transient gale of applause, care not whether they be finally lost on the sands of apathy and forgetfulness, or end their course on the broad expanse where new worlds, new ideas, open to their astonished minds.

True, indeed, we have not now to fear, in the promulgation of any new doctrine in philosophy or discovery in physic, the faggot and the stake, or the dungeon's gloom; though we are still opposed by intolerance, the offspring of pride and indolence, which, anxious to shroud itself under the veil of higher feeling, is ever ready to cry out, that religion and morality are in danger—that the social edifice will be shaken, in the attempt to substitute new and graceful columns, for the heavy and disjointed masses of discordant materials and disproportioned parts.

Taking a retrospect of the past, we shall have little reason to boast of the strength of philosophy, or power of discrimination displayed by the great and the learned, in their opposition to novelty, whether consisting of moral doctrines or scientific illustrations. Need I mention the prince of philosophers, who first introduced the knowledge of one great cause to the minds of his countrymen, benighted in the darkness of polytheism, and who, the object at first of their raillery, and subsequently of their hatred, could only expiate his imaginary crime by drinking the deadly draught. To what indignities, what tortures, did not the author of our holy religion submit, in teaching those pure and sublime

* Stewart's Elements of the Philosophy of the Human Mind, pp. 30, 31.

truths, which expand the mind, while they warm the heart, and which are now, as they ever will be, received as the lamp that lights our steps in this world, and guides us to the next.

In following the march of time we see the successors of Saint Peter—of those same men who, afraid to meet the light of day, used to celebrate their religious rites, and preserve their holy mysteries in dark caverns and subterraneous passages,* thundering from the Vatican their anathemas on the reformers of religion, and imprisoning a Galileo for opinions, now universally acknowledged as incontrovertible truths, which it were folly to deny; then, stigmatized as heretical and contrary to common sense and scripture.† Nor was he who substituted correct reasoning in philosophy for the pedantic sophistry of the Peripatetic school, more fortunate than his great cotemporary—and Descartes, now styled by his countrymen the reformer of the human mind, opposed and persecuted by the decrees of the Sorbonne, was compelled to end his days in exile, and seek that tranquillity in a foreign land denied him in his own. In approaching nearer our own times, we find the followers of this same philosopher, who was far less happy in his explanation of the phenomena of nature, than of the operations of the human mind, oppose with passionate warmth and ridicule, as absurd, the views and discoveries

* If, on ascending from the catacombs of Saint Sebastian, the cemetery of persecuted Popes and of tens of thousands of martyrs, in the passages of which were excavated at once couches for the living, graves for the dead; where the neophyte first received the sacred sign of the cross, and the saint encouraged his brethren to press on in their high calling, to win the crown of immortal glory,—we glance at the lofty dome of St. Peter's, towering in the heavens, and emblazoned with the wealth of Christendom; what an instructive moral lesson is conveyed! how forcible the appeal to charity and benevolence, to toleration and mutual indulgence: but alas! how mortifying the reflection, that this mute but eloquent instruction should have been so often overlooked and disregarded!

† The text of *Viri Galilæi quid statis aspicientes in cælum?* chosen by a preacher as the *argumentative* beginning of a laboured denunciation against the doctrines of Galileo, while that eminent man was confined in Rome, is no unapt specimen of more recent attempts at wit and reason.

of a Newton; and France was, after all, indebted to the poet and historian, for making her acquainted with the truths laid down by that great and good man.

Then, as now, the language of opposition was in the same strain—working on the timid through their fears, and the sensitive by ridicule—appealing, alternately, to names over whom time had thrown his venerable mantle, or authorities, which, by being blindly followed for years, were thought stable. But the most scrupulous need not hesitate in yielding their attention to those novelties which can only be met by general accusations for innovation, or satirized as occasionally lending to the ludicrous. Ridicule is generally an unlawful, as it too frequently has been a profane weapon, which, cutting both ways, may wound him to-morrow, who has triumphantly wielded it to-day. Fear may compress, but can never guide the energies of reason—and though justice occasionally use it for the preservation of external right, religion and pure morality disclaim its aid.

These observations naturally lead us to the subject for which we have now met—a subject not to be viewed as gratifying idle curiosity, or giving license for impertinent conjecture, but as involving consequences the most necessary to a correct study of the human mind, and exerting an important bearing on our social happiness.

Most of you are no doubt acquainted with the circumstance which first led Gall to the study of the mental faculties, as manifested by particular parts of the brain. This gentleman was first induced to consider the subject, from observing at college, that although he excelled many of his companions in powers of observation and reflection, yet they frequently bore away the prize at the examinations, from their better memory. By one, particularly, whom he was very anxious to excel, in this way, he found himself foiled, notwithstanding all his efforts, and his conviction that in other respects he was the superior. He, at first, attempted to find out the cause of this difference by studying the physiognomy of his companion, whose eyes were very prominent—and applying this sign to others, he discovered

that all those distinguished for their verbal memory possessed it. For a time he thought it indicated memory in general, but meeting with some persons who remembered very well the different places they had ever visited and their peculiarities, yet had not prominent eyes, he began to think there must be some other sign to mark this latter faculty, and observing attentively those who displayed it, he found in all a protuberance or fulness at the upper part of the internal border of each eye-brow. Among the busts that he took of these persons, was that of his young companion, named Scheidler, who possessed, in a very strong degree, this faculty, so as to remember the places where he had ever been before, and even to point out each bush, where, in their boyish days, they had found bird's nests, and this without any artificial sign. In him the organ was much developed. Gall met a woman one day in Vienna, in whom this part was so prominent, as to produce deformity of the face, and having interrogated her about her inclinations, learned that her greatest pleasure consisted in travelling. Experience has since confirmed, that the love of travelling, as well as the accurate remembrance of places and scenes, depends on the development of this organ. I could myself mention a strong proof of this, in a most intelligent gentleman, who rarely seems content unless on a journey or tour.

Extending his observations, Gall soon discovered part of the head much developed in those who displayed strong feeling and passions. A beggar, among others, acknowledged, that his pride had reduced him to indigence, and that from his very infancy, he thought himself superior to others, and would never learn any thing. The posterior and upper part of his head, where the hair usually divides, was found very prominent, and the same configuration was met with in others remarkable for their pride.

Thus furnished with so many proofs of coincidences between the development of different parts of the brain and the display of the intellectual and moral nature of man, Gall was compelled to renounce the obscure and incomprehensible doctrines of the schools, and applied himself to

the study of the primitive faculties. Having associated Dr. Spurzheim with him in his labours, they extended the sphere of observation, and visited the different cities of Germany and France, where, in the prisons and museums, as well as in society at large, they had ample means of verifying their opinions and adding innumerable facts to their support. At a later period, Spurzheim visited Great Britain, where, though the new doctrines were opposed with all the violence that abuse and ridicule could suggest, they have since gained advocates and supporters among the most intelligent in different professions, as is amply proved by the interesting essays of Combe, Mackenzie, Lockhart, and others—and by the establishment of a Phrenological Society of Edinburgh—in the very city where the opposition was at first most fierce and intolerant.

It is not my intention at this time to enter into a minute consideration of the objections urged against the study of phrenology, but I shall content myself with a notice of the most prominent, as with their refutation the others may be readily overlooked. And first, the very foundation is attempted to be subverted by a denial of the brain's being necessary to the manifestations of the mind: this indeed is now so untenable a position, that it is seldom taken, unless by those in whom metaphysical vapours have obscured the clearness of ideas which they might readily attain on the subject. There is scarcely a point better established, or more clearly demonstrated in physiology, than that a general development and healthy state of the brain are necessary for the regularity and strength of mental exertion. We shall draw proofs of this from numerous sources other than from Gall, Spurzheim, or their followers. Magendie, one of the first physiologists of the age, says, "The brain is the material organ of thought: this is proved by a number of facts and experiments." The committee of the French Institute, composed of Cuvier, Pinel, Portal, Sabatier and Tenon, appointed to report on the memoir of Gall and Spurzheim, though differing from them in some respects, say, "Experience has early proved that the brain is the

material instrument of our mind, and the essential organ of animal life." Those celebrated teachers in the Edinburgh school, Cullen and Gregory, have both announced the same opinion. The former says, " We cannot doubt that the operations of our intellect always depend upon certain motions taking place in the brain: and the latter, in his, *Conspectus Medicinæ Theoreticæ*, tells us that the brain is the primary organ of the intellectual senses (meaning memory, imagination, judgment.) Willis, Haller, and Blumenbach, announce similar opinions. But why dwell on authorities, however great their judgment or celebrated their genius, when daily instances are met with in every large city, of extinction of intellect from injuries of the brain by external violence, producing fracture, and depression of the bones of the cranium, or by effusion of blood or serum in its cavities, as in apoplexies.

We are told indeed that there are instances of the powers of the mind being perfect, when there has been a loss of part of the brain, and nearly an obliteration of it in some cases. That persons who have thus suffered have not become, in consequence, idiotic or insane, we can readily believe ; though much greater accuracy of detail and nicety of observation are required, before we can be satisfied of their memories, imagination, or judgments, not having been impaired. Between the active exertion of a faculty, and its passive operation, there is a wide difference, which our common language does not sufficiently express, or our observations mark. We may say of a man recovered from a violent disease which had interested some of the vital organs, that he is as well as he ever was, or of another who has had his ancle sprained, and recovered the use of his limb, that he can use it now as before—Yet let the former be ever so little exposed beyond his usual custom, or the latter attempt the race in which he may have before excelled, and both will tell you that though they be well for the world or their friends, they are not as they used to be. All that we wish inferred from these remarks is, that the exceptions are too few, too vaguely described, to have

the necessary weight in opposition to the principles we advance: nay, more—in proportion as we minutely observe the effects of these morbid changes on the brain, we shall have additional proofs of the correspondence between cerebral development and mental manifestations. The brain being double, accounts for the operations of mind going on through this organ, though it may have lost part of its substance, in the same way as the ideas formed of colours and external objects by the instrumentality of sight are nearly as perfect with one eye as with two. The power of accommodating itself to the changes induced by slow disease, with which Providence has invested all organized matter, is no argument against the functions attributed to different organs. We are not the less satisfied of the lungs being the organs of respiration, and of the full size and integrity of the pulmonic apparatus being necessary to the preservation of health, from the circumstance of one lung having been gradually destroyed by ulceration, without the person suffering much inconvenience. A longer explanation of this point would lead us to anatomical details, which for the present we are desirous to avoid.*

* This waving the discussion of the subject proceeded from the circumstance of many of the members of the society being gentlemen of the bar, or of other professions, who, I presumed, were not yet acquainted with the anatomy of the brain. The subject which I here passed over was one at first urged with an air of triumph, as an insuperable objection against the position we now advocate—but which, by more recent and accurate observations, goes to prove the superior anatomical and physiological knowledge of Gall and Spurzheim; and confirms their phrenological views—I speak now of dropsy of the brain or *hydrocephalus internus*. The accumulation of fluid in the ventricles gradually distends them, and insinuating itself between the medullary or internal surfaces of the convolutions, gradually unravels, as it were, these latter, and reduces the brain to the appearance of a sac, a membrane filled with fluid: the cranium in the mean time accommodates itself to the pressure from within, and becomes enlarged sometimes to an enormous extent, three to four feet in circumference—but without, as was once asserted, the sutures being opened, or the ossification being incomplete.

This shows that the enlargement of surface compensates for thinness of structure of the brain, and that though altered in form, it is not ne-

In idiots the brain is found deficient, and though not of any specific form or size, is always deformed. Who among you has seen one of these unfortunate beings, that had not at the same time defective cerebral development. We know that too small a head is one of the obstacles which prevent the manifestations of the soul. Willis of London, Bonn of Amsterdam, Pinel and Esquiro of Paris, all describe idiots in whom the brain was exceedingly small, and imperfectly developed. On the other hand, those who have been distinguished for their great talents—Bacon, Leibnitz, Haller, Bossuet, Voltaire, &c., and in our country, Franklin, had all voluminous brains. It will be afterwards seen, that though this be a general criterion of intellect, we must specify the part developed, to determine, whether intellect or passion most abound. We say nothing of those who have some single talent in a high degree, for they might have this, with a brain of a very moderate size, but we shall find the development of the organ corresponding to the particular talent. To Magendie we again refer: in general, says this experimentalist, the volume of the brain is in direct relation with the mental capacity. When, however, we speak of a large brain being favourable to the display of the intellectual faculties, it is but fair to hold in mind that we are to be understood as alluding to one with some degree of symmetrical proportions—in the same way as when we speak of the athletæ, gladiators or pugilists, as large powerful men—size, though a general requisite for success, is not the only one; and nobody would tell us that large men are not strong, because some individuals, loaded with fat, or puffed out with dropsy, have size but no strength. In some idiots there is a considerable mass of brain, but we shall draw from this circumstance an additional proof in favour of our system, when we come to speak of the plurality of organs.

cessarily absorbed or destroyed, and may on occasions be adequate to the usual employment of its faculties. Gall and Spurzheim have unravelled, as it were, the brain, and produced an appearance somewhat analogous to that described above as resulting from disease.

Admitting then, say our opponents, that the brain is the seat of the mind, and that on its perfect development depends the perfection of mental power, still we cannot tell from the external configuration of the skull or cranium, the size of the brain. This is an objection urged with much eagerness and apparent plausibility, but is met by the physiologist just quoted, who tells us, that the only method of estimating the volume of the brain in a living person, is to measure the dimensions of the cranium; every other method, even that proposed by Camper, is erroneous—The Edinburgh Reviewer with all his warmth of opposition says: "but we will acquiesce implicitly for the present in the proposition, *(familiar to physiologists long before the age of Gall and Spurzheim,)* that there is in most *instances a general* correspondence between the size of the cranium, and the quantity of cerebrum," (brain). This once admitted, we might rightly infer, that the correspondence of the figure of the skull, and development of the brain, will be found in particular parts, as well as in the whole structure. Spurzheim used to prove this, by using the skull as a mould, for casts of plaster of Paris, on which the correspondence between their external surface and that of the skull, was clearly displayed. Doctor Harlan, of this city, has satisfied himself of this coincidence by similar means.

The last and expiring effort of metaphysical obscurity, against observation and inductive reasoning, is in the denial of the innateness and plurality of faculties, and of their corresponding organs—It will tell us that the soul or the mind sits enthroned, and acts by various ways to us unknown; sometimes showing its strength in making this man a poet, another a mathematician, a third a painter, by certain processes called perception, memory, imagination, judgment, &c.*—how, we are not to inquire, or why there is such a

* That is by faculties, (assuming for the moment that they are such,) common to man and brutes. That the latter have perception and memory, is acknowledged by all: their dreams—their "lively conception of objects of sight," the ability of some to accommodate themselves to peculiar circumstances and situations, of course making comparisons,—prove their

woful discrepancy in the definitions and deductions of philosophers, who have treated this subject: so that except acquiring a few fanciful thoughts and metaphorical allusions to images, sympathies, associations, &c. those persons who have most studied the common works on the philosophy of the mind, will tell you there is nothing left, nothing to which they can attach definite meanings to guide them in the study of human nature. These speculations go about as far to explain the operations of intellect and feeling, as the old doctrine of the horror of a vacuum did in accounting for several natural phenomena. We smile at this language now in natural philosophy,—the day will come when we shall smile with similar feelings at our present systems of moral philosophy.

Though the plurality of faculties, and their being assigned corresponding seats, seem so repugnant to the mysticism of the present school of philosophy, it is nevertheless an idea that had been advanced by many physiologists, and even some divines, but was supported by few data, and no attempts were made to fix the seats of the faculties or observe the coincidence between cerebral development and mental manifestations, before the time of Gall, who is entitled to all the credit of original discovery and successful applications.

The analogies of nature and the operations of the mind, all go to prove the brain's being divided into different organs. Each salt and mineral, has its peculiar crystallization: each kind of tree is differently organized, as also its differents parts—the wood, leaves, flowers and fruits—The organization of each animal is equally modified, and in the

possessing imagination and judgment. Locke gives brutes the powers of comparison and composition—that is, the combination of the above primitive faculties as they are incorrectly termed, but endeavours by metaphysical subtilty to establish a difference between man and them, by denying them abstraction—"an excellency which the faculty of brutes does by no means attain to." The following up this distinction, requires so much abstract belief in lieu of comprehension, that I must acknowledge my inability to do it. We shall apply these facts as we advance.

same animal each function is attached to a particular organ, as the heart to circulate the blood, the lungs for respiration, &c. In the nervous system we see the five senses separate and independent of each other: there are as many instruments as kinds of impressions. More direct proofs are furnished by the actions of man and animals in a state of health and in disease. As respects the latter, their brains must differ in the totality, since the qualities of these animals depending as we have shown on the brain, are unlike. The beaver that builds a hut, the dog that hunts, the tiger crouching for his prey, the docile elephant, cannot have the same constituted brain. The different individuals of a species, never possess the faculties in the same degree; some are distinguished by their peculiar qualities,—others are stupid,—consequently the organization cannot be equally perfect. There must also be modifications in the cerebral development of the two sexes, for we know that some faculties are most active in women, others in men. But we have only spoken of the brain as a totality. If it were not composed of different organs, why do we find in the scale of beings that it is more complicated in proportion as faculties are multiplied.

In our own species we see at an early period, in infancy and tender age, one part of the brain well developed, the other only becomes so at a later period: the first, composed of the upper and posterior part, enables them to show at once most of the passions which mark the more mature man: while it is only with the gradual and more perfect development of the anterior portion, that the intellectual faculties are displayed, and assume a controlling power over the former. Here then in the healthy state we have two grand divisions of faculties and corresponding organic structure marked out. In those unfortunate creatures who have never displayed the usual signs of intelligence, and whom we term idiots, there is not simply imperfect development, but absolute deficiency of organization,—the anterior part of the brain seems wanting, and its height is much below the common standard—yet many of these be-

ings display the lower propensities strongly, while the higher feelings and intellect are wanting.—If brain were an unit, ought we not in these idiots, as in children, to see a proportionate activity or a proportionate feeble display of passion and intellect, of feeling and reason.*—That such is not the case we all know, but finding that certain propensities and passions exist in all their energy in the infantile state, as in idiocy, whether in casual cases or among the cretins of the pays de Vaud—and accompanied with a certain development without which they would not exist,—we must assume the latter as a distinct and separate cause, from that producing intellect or higher reasoning powers, which we observe accompanied with a development of another part of the brain, and which may render them very active without any marked display of passion or strong feeling. Here are two sets of faculties, two sets of organs, which every man, who will give himself the least trouble of observation, must admit. The charm of unity once broken, he will not stop at simple duplicity, but will soon satisfy himself with the plurality of faculties and their organs.

In the same person certain inclinations and certain faculties are manifested with much energy, others very feebly. One man may have much verbal memory and little reasoning power; he may be a great poet and no general; a good painter and no mathematician. He may be devout and either stupid or intelligent; each has his gifts,—consequently a similar mass cannot be adapted to all these functions. If there were but one external sense, it might suffice to perform the offices of the whole; it ought to see, hear and smell; but since the functions of the external senses are attached to different organs, they may exist separately, and one may be very strong while the other is very weak. It is the same with the internal senses. If the same organ were destined to manifest all the faculties, how could the soul exercise with

* See the representations and descriptions of such beings in Pinel *sur l'alienation mentale,* and in Spurzheim on Insanity, also Richerand's Physiology under the head of Sensations.

this instrument such a faculty in perfection, and such other in a limited degree; or why should not all the faculties increase in activity, or decrease at the same time—whereas some, as already remarked, are very energetic in children, others only displayed in adult age,—some decline at forty or fifty—others last till decrepitude comes on.

How can we, on the commonly received principle, account for the mind, fatigued with continued attention to one subject, applying itself to another with vivacity and pleasure. The plurality of organs explains this, in the same way as when the eye is fatigued with looking at brilliant colours, or varied landscapes; and almost incapable of action, the ear may still be flattered by agreeable sounds. The state of sleep and dreaming affords proofs to the same effect. If the brain were one organ, there must be either watchfulness or entire repose—perfect sleep, whereas we know that in some persons the mind is very active during this state, calling up a variety of imagery and succession of events. The circumstance of the subjects of our dreams seldom occupying much of our attention during our waking hours, and of those subjects most studied being least dreamed of, is readily explained on the principle of the faculties which these latter kept in exercise, being fatigued and requiring complete repose; whereas the others, little thought of during the day, and the faculties on which they depended having been little exercised, do not repose like the others, but rather disport themselves by enlarging and combining new ideas. It is with the faculties of the mind as with those of the body; exercise must be followed by rest or sleep, which is more complete the more active and universal the exercise has been; but if this be carried beyond a certain point, that of simple fatigue in either, and the faculties are overtasked, as in the body by excessive muscular exertion, in the mind by very intense study, strong fear, disgust, or great anxiety and grief,—fever, restlessness and tossing will ensue in the former, and dreams partaking of the predominating feeling in the latter. The modifications of dreams produced by the different periods of life, afford ad-

ditional illustration. In early age and youth, our feelings or propensities give the impress to our dreams; in manhood, the intellectual faculties being matured, divide the night with the former, while, in old age, the same faculties being enfeebled, the dreams run back to childhood and early life.

Somnambulism affords another instance in favour of the plurality of organs. We know that the brain acts on the external world through the medium of the senses, the voice, and the instruments of voluntary motion. This may be partially displayed in sleep, according to the number or energy of the cerebral organs that are active; some dream and speak; others dream, speak, and even hear; others again, in their dreams, will get up and perform various actions; it is generally this latter state that we term somnambulism. Visions, inspirations, hallucinations, are only explicable on the same principle of the plurality of organs.

It is impossible to reconcile the facts of partial insanity, or monomania, with the notions of the unity of the brain. Some persons are, we know, only deranged on one subject, and display in other respects a consistency of conduct, and occasionally a strength of reasoning, that would deceive a stranger. So in the external senses, sight may be destroyed, and hearing and touch become more acute than ever.

But if mind must be an unit to coincide with unity of consciousness, we can readily admit it, in the same way that life or the vital principle is said to be an unit; and as the latter is modified by different organs, each having its peculiar modes of action and function, so is the former; and digestion and respiration, sight and hearing, are not more distinct modes of action, than verbal memory and causality, or local knowledge and ideality.

The weight of reasoning is decidedly in favor of the opinion of a plurality of faculties and organs, and we should be perfectly justifiable in regulating our studies and views of human nature accordingly, and entitled to give our science the name of Phrenology; but it is by observation

and experience that its advocates propose testing its truth, and displaying conclusive proofs in its favour. Happily here is no appeal to credulity, no attempt to bewilder the mind in the mazes of metaphysical conjecture; the unprejudiced man has but to study the basis of the doctrine, and look abroad on the world, for the demonstration of its correctness. Conviction must be the result of such conduct. Then will he regret his former levity, and regard with pity and sorrow, the feeble attempts of those who substitute vague conjecture for observation, and ridicule the harmonies of nature and the work of God.

We must not here omit noticing some other objections made to this doctrine, not indeed deduced from *à priori* reasoning, or experimental observation, but from presumed final effects. Though they be sometimes urged with art, and with an intention to alarm where they cannot convince, yet as occurring to those whose amiable sensibilities would make them shudder at being led on to the adoption of any opinion that might lessen their exalted veneration for the Deity, we shall pause for a moment to show the unreasonableness of their fears, and satisfy them that a very contrary effect will follow their studies on this head. It is not a little singular, that almost every philosopher, whether in morals or physics, has been in his day charged with supporting materialism—much too substantial a term for so vague and unmeaning an accusation.* Locke, when he denied that there were any innate ideas, was charged with materialism. The Phrenologist, in contending that man is born with innate faculties and propensities, labours under the same accusation. The former was at first said to be an atheist—a deist. The same charitable insinuations have been thrown out against the latter. The opinions of Locke have, however, been received and commented on by philo-

* The following explanations are intended for those who really have sensibilities for the moral tendency of any doctrine, and reason enough to be convinced when suitable arguments are used. As to the crowd, it will be sufficient to ask what they mean by materialism and fatalism.

sophers and divines. Ere long the same honours will be conferred on Phrenology, with so much the greater probability of permanency and utility, as its doctrines are founded on the intimate constitution and nature of man.

When we say that mind is displayed through the medium of the brain, we do not say that mind and brain are one, any more than that life and flesh are the same, though for the display of both mind and life, a certain organization is necessary, in this our present state of existence.

"So far, the properties which we have discovered in bodies were constantly inherent in them, and seemed essentially attached to the matter which composes them. It is thus that ponderable bodies cannot be deprived of weight, nor their molecules of the property of mutual attraction." (Such is materialism.) "We are now going to examine other kinds of modifications, which may be temporarily impressed on bodies, and which are so much the more singular, as without adding or taking away from their particles any tangible or ponderable principle, they develop nevertheless very powerful actions, the mechanical influence of which may subsequently put in motion material agents." *Precis Elementaire de Physique Experimentale, par* J. B. Biot. Tome I. p. 403—4. The language of this philosopher, you will doubtless imagine, has reference to the nature of mind and life—by no means—it is the introductory notice to the chapter on electricity, which, as well as galvanism, magnetism, and even light and caloric, we know as little of in the abstract as of mind itself. It is only when acting on matter, or combined with it, that we become sensible of its presence. Still are we enabled, by an attentive observation of its conductors, and its mode of transmission, to lay down some general rules on the subject, and to render it not simply subservient to curiosity, but a powerful means in the hands of science, for laying open some of the hitherto concealed operations of nature. So of mind in the abstract, we can only conceive it as emanating from the Deity, but like every thing here below, displays its powers by certain modifications of matter. Light and electricity

are not changed in themselves, but may be rendered more or less apparent or active, according to the nature or quantum of the body employed for collecting or conducting them. Is it not more natural to suppose mind as coming from the Deity, and intrinsically the same, though favoured or impeded in its display by a more or less happy organization, and by health and disease, than to imagine an immaterial principle, independent we are told of matter, yet sometimes puny, or sick, altering its state like the stomach; surely this fashionable idea savours most of materialism, or at least tends most to degrade the dignity of mind, and diminish our exalted admiration of omniscience. If indeed asked, why there should be a necessary connection between the mind and the brain, we cannot tell: we only attempt to go a link higher in the chain of causation: the first cause is hidden from us; as it ever has been, and ever will be, in this world from all others.

The second grand objection urged against phrenology, is its supposed tendency to fatalism. We shall not, as we might, rest our answer on the principle that what forms part of the creation and enters into the plans of the Deity cannot be wrong, and that we have only to examine and take things as we find them, not ask why they are so. On attentively observing the march of things, the operation of cause and effect, of actions and motives, we shall discover that man has strong animal feelings and propensities, which may, and unhappily often do, impel him to the commission of crimes, destructive of his own peace and the world's welfare. The gloomy prison and the triumphal arch—the last abode of the ignoble robber, the proud memorial of the haughty conqueror—are but too striking proofs of this melancholy truth, acknowledged by the philosopher and deplored by the saint. But that man, a world in himself, should be necessarily or irresistibly led away by these meaner propensities, must be denied by all, and by none with more zeal than by the advocate of phrenology, who points out higher and nobler faculties, by which we are impelled to adore a Supreme Being and know what is due to our spe-

cies; which prompts us to kindness to our fellow men, and carries us beyond the confines of the present, looking with eagle gaze to the light of futurity. All these, while they tend to elevate the natural man above the brute, enable him to receive the truth of revealed religion, and joined to his intellectual faculties, give him a triumph over his animal propensities, in the same way as he is able to control and regulate the different races of animals for his own use—and he may oft exclaim,

> "Yet with my nobler reason, 'gainst my fury
> Do I take part."———

Phrenology here is in accordance with general experience, by showing that these higher or affective faculties, with their corresponding organs, are most constantly found, and are independent of the varied intensity of the animal propensities or activity of the intellectual faculties. That some are deficient in their means of judging, or have not the higher faculties active, cannot be denied;—why it is so we know not.* In what happens against our will there is one kind of fatalism entering into the plans of the Creator. We belong to families rich or poor—A child may inherit the diseases of his father, and thus be punished for what his actions did not merit. Some persons are born blind, a fate they no doubt consider hard; so in the same way people are born with more or less feeling and intellectual power: there are born stupid men and men of talents, so far there is fatalism. But we shall be told that these lower propensities will lead us away—The power being given, it does not follow that it should inevitably act. The dog is hungry, yet may be restrained from eating, and we punish it for the at-

* There seems to be less of fatalism, after all, in this condition of things than that supposed by the school of Locke and Stewart, which gives man powers possessed by the brutes, and on which they are solely dependent for their moral and intellectual improvement and advancement, which is more or less perfect according to the education received, and the opportunities presented for association of ideas; circumstances over which the mass of mankind have as little control, as of the dispositions which they may be born with. It is needless to ask which is the doctrine most favourable to natural and revealed religion.

tempt. Experience shows of our internal powers, that they may exist without action; hence, a kind of liberty not universal indeed or unlimited, for all that is created is subject to certain laws. No man acts without motive. What are the bonds of friendship, the calls of religion, if motives do not regulate us. Three conditions appear necessary to make a being free. First: Several motives, if one only exist, there is no choice, so that we are accustomed to speak of the animals guided by only one motive, as acting by blind instinct. The greater the number of motives the greater the liberty. Second: There must be power to choose, that is, the understanding must have power; many confound pleasure with liberty: the mere liking any thing is not a sign of freedom. The tiger, pleased with destruction, follows his inclination in pursuing its prey. Legislators admit the necessity of the understanding's being developed before persons can come under the influence of laws; hence children and idiots, though possessed of very energetic desires, are exempt from their operation. Third: The will must have influence on actions, but never can it have on feelings—We cannot suffocate the latter, but we can refrain from gratifying them. Sometimes the will loses its power over its instruments; we call this state insanity. Moral freedom then consists in man's acting from motives peculiar to himself and not to animals. Now we see by phrenology, that he has organs and corresponding faculties which animals have not, consequently should say that he has moral freedom. We shall give in this place, an extract from Spurzheim's *Essai Philosophique sur la Nature Morale et Intellectuelle de l'Homme.*

"It results from this section, that the organization of man is not sufficient to explain the functions of animal life; that these latter are subject to certain laws; that consequently, man does not enjoy unlimited liberty, but liberty founded on three conditions; the plurality of motives, understanding to make choice of them, and the influence of the will over the instruments necessary to acting; that man alone has a *moral* liberty by means of certain faculties which are peculiar to him, and which constitute human nature; that

every thing conformable to it is good, and the contrary bad; that evil in general results from the infraction of the social order; that natural morality consists in submission to the laws of the creation, and in the general happiness of the species; that the obedience of man to the order established by the Creator constitutes his merit; that natural and christian morality are but one; that the knowledge of human nature contributes to display the excellency of christianity; that the existence and necessity of moral laws are not simply founded on faith, but also on proofs palpable, visible and inherent in man; finally, that morality is simple, universal and invariable as the species, and that it ought to be the basis of every institution for the preservation of social order." p. 135. Hear what the celebrated Doctor Chalmers says in one of his sermons. "God has, for the well being of society, provided man *with certain feelings and constitutional principles of action*, which lead him to a conduct beneficial to those around him, to which conduct he may be carried by *the impulse of these principles*, with as little reference to the will of God as a mother among the inferior animals, when constrained by the sweet and powerful influence of natural affection to guard the safety and provide for the nourishment of her young."*

Seeing also that no two persons are precisely alike in disposition or intellect—must we not renounce a little of our own will in intercourse with others, to preserve the necessary harmony—we shall then be more tolerant and indulgent—Christianity recommends it, and it is in nature.

Returning now from the moral and philosophical views which the new doctrine opens to us, to the consideration of the physiology of the brain, we find great stress laid upon the circumstance of persons having received injuries of the brain or having lost particular parts of it, without, as has been alleged, their dispositions or characters having

* For a continuation of this discourse, see the Appendix to Combe's Essays on Phrenology, now in the press, and shortly to be published by Messrs. Henry C. Carey and I. Lea. In this work are found the opinions of other divines perfectly conformable to those here advanced.

undergone any change. But we must ever hold in mind, that this is only negative reasoning, destitute of precision and of that philosophical character which ought to sway our judgments. How difficult to determine, how impossible to be observed, unless a man's attention be directed exclusively to the subject—the varied shades of aberrations of intellect or modifications of feeling—from the trivial display of caprice to entire mental derangement and storm of ungovernable passion. It has been wisely ordained by the Deity, as well for the purpose of keeping our minds in constant action, as of making us feel our dependence and littleness, that in all our reasonings, in all our systems, when we flatter ourselves at having given them the greatest consistency, and formed them with the greatest harmony, we are met by some exceptions, something out of the usual order of things, which makes us exclaim, not—all is faulty—but, we know not all. Shall we reject the explanations of the natural philosopher and chemist, and deny the force of his experiments on the expansive powers of heat or caloric, because water, which is taken as a common, and in general a correct illustration, presents in some of its changes exceptions to his system. Is yonder meteor shooting across the heavens to make us renounce our belief in the Newtonian doctrine of gravitation and repulsion, or lessen our admiration of the harmonious movements of revolving suns and worlds? What should we say to the flippant youth who, on hearing his professor explain the structure of the human frame, and the different parts of which it is composed, as all necessary to health and perfect life—starts up and exclaims, "Sir, you must be wrong; I have seen persons enjoy health who have not had all these parts you speak of."

But in place of negative reasoning and conjecture we can adduce positive facts, showing the constant correspondence between the development of an organ and the propensity: and other instances equally well attested by persons above suspicion, and who had no particular theory to support, of wounds of the organ being followed by

the loss of the corresponding inclination, and paralysis of function.*

We find the organ of philoprogenitiveness, or love of children, is well marked in those persons, particularly mothers, whose tenderness for their offspring absorbs all other feelings and affections. We should of course expect to find it small or nearly wanting in those who neglect their children or barbarously destroy them, and such is the case. Spurzheim has seen thirty women guilty of infanticide; twenty-six had the organ very slightly developed—the others were carried away by the violence of particular circumstances, and destroyed their children—One went immediately and told what she had done, urging her misery as the cause.†

Dr. Stack of Bristol, in England, formerly a pupil of Dr. Rush, was made a convert to phrenology by discovering this organ almost wanting in a woman who had committed infanticide; that is, it put him in the path of inquiry and observation, which resulted in entire conviction. It is not to be inferred, that the want of this inclination leads a woman to destroy her offspring, but if it be illegitimate, she has one motive less for avoiding crime.

The peculiar situation of the mother as the nurse of her child, does not satisfactorily account for her greater tenderness for it. At a very early period we find a marked difference in the amusements and likings of the two sexes, only to be explained by a difference in their cerebral organization; hence we see the head of the female generally longer than that of the man. How often do we find the mother treat her children with excessive indulgence, in spite of all remonstrances, and even of her own reason. In one case of

* In addition to facts of this description witnessed by Gall himself, we have others furnished by Larrey in France, Formey in Berlin, Hennen in England, on the consequences of wounds of the cerebellum or posterior part of the head.

† See Spurzheim's "Phrenology." If we look at the changes induced in the brain from alterations in structure in man and animals, we find additional confirmation.

a poor woman with several children, in whom the above organ was largely developed, on its being remarked that she must be fond of them, "Oh, sir," replied she, "what comfort would we poor people have but for our children." In different races and nations this feeling is different; the negro women have a great affection for their children.

The same marked difference is found in the two sexes in the brute creation. Sportsmen know the females of many animals, such as wolves, deer, hares, and rabbits, by their long heads. We might go on multiplying examples, but must reserve farther illustrations for a future period.

Few men have been more exposed to obloquy and abuse than Doctors Gall and Spurzheim, and few have been able so successfully to oppose the knowledge of facts and inductive reasoning, to the vague and contradictory assertions of their antagonists. Avowedly the best demonstrators of the brain that have ever lived, they have thrown new light on its structure, and have elucidated some points in its pathology hitherto deemed inexplicable.* They have shown themselves as profound and able reasoners as expert anatomists; and extending their observations from the prison of crime to the closet of genius, have accumulated such a mass of facts and illustrations, as to enable them to reply confidently to their opponents—examine yourself, look around on the world and be convinced.

The applications of the system are numerous and diversified. The subject on which it is peculiarly calculated to throw a powerful light, are Education, the Philosophy of Criticism, Criminal Legislation and Insanity.

That we may be enabled to avail ourselves of such lights, by acquiring the necessary knowledge of the science, as well by communicating our respective observations, as by opening a correspondence with Edinburgh and Paris to procure additional illustrations, has been the object of our

* For a brief account of their mode of displaying the brain, and their views of the nervous system, see Combe's Essays, already cited, in which will be introduced a paper on the subject.

union; and let me hope in conclusion, that the society will, without delay, proceed to so important a work, amusing and instructive to ourselves, as we may afterwards render it beneficial to others.

II.

Itaque minime mirum est, si scientiæ non crescunt cum a radicibus suis sint separatæ. Nov. Organ. And in another place, as a corollary from this, the author says, *vere scire, esse per causas scire.*

How can we pretend to study the operations of the mind, if we neglect the means through which it manifests itself. A sure proof of the imperfection of our knowledge on any subject, is our resorting to one general term to express a succession of phenomena, each of which would, of itself, demand a precise and distinct definition. Hence the terms understanding or reason, as applied to the actions of man and instinct, in reference to those of animals, have done much to retard our progress in the study of the philosophy of mind.

In opening the subject this evening, I shall quote from a writer who appears to have examined for himself, and who, were he now living, would be called a phrenologist. "Many theories," says he, "have been invented with a view to explain the instinctive actions of animals; but none of them have received the general approbation of philosophers. This want of success in the investigation of a subject so curious and so interesting, must be owing to the operation of some powerful causes. Two of these causes appear to be, *a want of attention to the general economy and manners of animals, and mistaken notions concerning the dignity of human nature.* From perusing the composition of most authors who have written upon animal instinct, it is evident that they have chiefly derived their ideas, not from the various mental qualities discoverable in different species of animals, but from the feelings and propensities of their own minds. Some of them, at the same time, are so averse to allow brutes a participation of that intellect which man pos-

sesses in such an eminent degree, that they consider every animal action to be the result of pure mechanism. But the great source of error on this subject, is the uniform attempt to distinguish instinctive from rational motives. I shall, however, endeavour to show that no such distinction exists, and that the reasoning faculty itself is a necessary result of instinct."

"The proper method of investigating subjects of this kind, is to collect and arrange the facts which have been discovered, and to consider whether these facts lead to any general conclusions. This method I have adopted, and shall therefore exhibit examples of pure instinct, of such instincts as can accommodate themselves to peculiar circumstances and situations, and of instincts improveable by observation and experience. Instinct may be defined: "Every original quality of mind which produces particular feelings or actions when the proper objects are presented to it."* The brute creation affords innumerable examples of pure instincts. Insects invariably deposit their eggs in situations most favourable for hatching and affording nourishment to their future progeny. Butterflies and other insects whose offspring feed upon vegetables, uniformly fix their eggs upon such plants as are most agreeable to the palate and constitution of their young. Water insects never deposit their eggs upon dry ground. Some species of animals look not to future wants. Others, as the bee and the beaver, are endowed with an instinct which has the appearance of foresight. They construct magazines and fill them with provisions.

"*Of the Instincts which can accommodate themselves to peculiar circumstances and situations.*

"To this class many human instincts may be referred. But as these instinctive propensities are likewise highly improveable by experience and observation, examples of them will fall more naturally to be given under the third class. Those animals are most perfect whose sphere of

* A definition which will apply to the innate faculties as recognized by phrenology.

knowledge extends to the greater number of objects. When interrupted in their operations, they know how to resume their labors and to accomplish their purposes by different means. Some animals have no other power but that of extending or contracting their bodies. But the falcon, the dog and the fox, pursue their prey with intelligence and address. The ostrich has been accused of unnaturalness, because she leaves her eggs to be hatched by the heat of the sun. In Senegal, where the heat is great, she neglects her eggs during the day, but sits upon them in the night. At the Cape of Good Hope, however, where the degree of heat is less, the ostrich, like other birds, sits upon her eggs both day and night. I had a cat that frequented a closet, the door of which was fastened by a common iron latch. A window was situated near the door. When the door was shut, the cat gave herself no uneasiness. As soon as she tired of her confinement, she mounted on the sole of the window, and with her paw dexterously lifted the latch and came out. This practice she continued for years."

"The third class comprehends all those instincts which are improveable by experience and observation."

"The superiority of man over the other animals seems to depend chiefly on the greater number of instincts with which his mind is endowed. Traces of every instinct he possesses are discoverable in the brute creation. But no particular species enjoys the whole.—On the contrary, most animals are limited to a small number. This appears to be the reason why the instincts of brutes are stronger and more steady in their operation than those of man. A being actuated by a great variety of motives must necessarily reason, or in other words, hesitate in his choice. Its conduct therefore must often waver, and he will have the appearance of being inferior to another creature who is stimulated to action by a smaller number of motives. Man accordingly has been considered as the most vacillant and inconsistent of all animals. The remark is just, but instead of a censure it is an encomium on the species. The actions of a dog or a monkey, for the same reason, are more various, whimsical and uncertain, than those of a cow or sheep."

"Most human instincts receive improvement from observation and experience, and are capable of a thousand modifications. This is another source of man's superiority over the brutes. When we are stimulated by a particular instinct, instead of instantly obeying the impulse, another instinct arises in opposition, creates hesitation, and often totally extinguishes the original motive to action. The instinct of fear is daily counteracted by ambition or resentment, and in some minds fear is too powerful for resentment or any other instinct we possess. The instinct of anger is often restrained by the apprehension of danger, by the sense of propriety, and even by compassion. Sympathy, which is one of our most amiable instincts, frequently yields to anger, ambition, and other motives. The instinct or sense of morality is too often thwarted by ambition, resentment, love, fear, and several of what I call modified or compounded instincts, such as avarice, envy, &c."

Though the writer be not entirely correct in his division of primitive instincts or innate faculties, yet he has gone far enough to show how naturally such opinions follow careful observation, and how astonishing that they have not been long ago more fully and clearly established. He then goes on to say: "The minds of brutes, as well as those of men, have original qualities destined for the preservation of the individual and the continuation of the species. The calling forth of these qualities is not instinct, but the exertion or energy of instinct. Instincts exist before they act. What man or brutes learn by experience, though this experience be founded on instinct, cannot with propriety be called instinctive knowledge, but knowledge derived from experience and observations. Instinct should be limited to such actions as every individual of a species exerts without the aid of either experience or imitation. These qualities and instincts vary in particular species; some are endowed with many and others with few; in some they are stronger, in others weaker; and their strength or weakness seems exactly proportioned to their number. *The difference of talents among men who have had the same culture, arises*

from a bluntness or absolute deprivation of some original or mental instincts. Taste, or love of particular objects, whether animated, inanimated, or artificial, is in some men so obtuse, that we often say it is entirely wanting."——

—— "There is, perhaps, a greater difference between the mental powers of some animals, than between those of man and the most sagacious brutes. *Instincts may be considered as so many internal senses, of which some animals have a greater and others a smaller number.* These senses, in different species, are likewise more or less ductile; and the animals possessing them are of course more or less susceptible of improving and of acquiring knowledge."

The author then devotes a few paragraphs in answer to the assertion of Buffon and some others, that animals are mere machines; a notion, "perhaps, too absurd to merit refutation," and concludes the chapter on instinct thus: "From the above facts and reasoning, it seems to be apparent, that instincts are original qualities of mind; that every animal is possessed of some of these qualities; that the intelligence and resources of animals are proportioned to the number of instincts with which their minds are endowed; that all animals are, in some measure, rational beings; and that the dignity and superiority of the human intellect are necessary results, not of the conformation of our bodies, but of the great variety of instincts which nature has been pleased to confer on the species."

Most of you, gentlemen, might imagine that I was giving you the language of Gall and Spurzheim, rather than that of the ingenuous Smellie, from whose Philosophy of Natural History, written at the suggestion of the celebrated Lord Kaimes, I have quoted the preceding remarks. Though authorities are but of secondary importance to those who have the means and the inclination to make their own observations, we shall here give the sentiments of another celebrated writer, for the purpose of widening the field of inquiry, and out of indulgence to the indolently sceptical, who may think, and perchance persuade others, that the doctrines now taught, are entirely new, unheard of, and not only con-

trary to the dogmas of philosophy, but unsupported by general reasoning and facts. Cabanis sets out with the principle that the connexion between the physical study of man, and that of the manifestations of his understanding, the relation of the systematic development of his organs to the analogous development of his sentiments and passions, show clearly that physiology, the analysis of ideas and morals, are only three branches of one and the same science, which may be most appropriately termed the *science of man*, or the anthropology of the Germans. In acknowledging with Locke and Condillac, that our ideas are the result of sensation or impressions made through the medium of the senses, he still, in conformity with the opinion of those philosophers, who prefer observation to simple abstraction, shows that all the determinations of animals and of man in early age, cannot, as Condillac thought, be the result of a reasoning choice, and consequently the fruit of experience; but are the effect of certain inherent powers, designated by the term instinct. He cites familiar instances of instinct in the brute creation, establishing clearly his position. Animals born blind know where to find their nourishment. Haller has seen several species, such as the young of the sheep and goat, at the very moment after birth, go and seek the mother at a considerable distance, before any experience could have taught them to make use of their legs, or have given them the idea that their mother alone could furnish them their wants. The young chickens of the great family of the gallinaceæ walk immediately on issuing from the shell, and run diligently after grains and peck them, without committing any error in optics. Kids and lambs butt each other in sport, before their horns have shot out. But of all the inclinations or propensities which it is impossible to refer to the lessons of judgment or habit, maternal instinct is assuredly the strongest, the most dominant. But the period which precedes maternity, displays to us, in animals, a series of actions which are still more inexplicable by the theory of Condillac. At this period, all the species are occupied with the sentiments and pleasures of love; still the birds, in the

midst of their songs of gladness, and some quadrupeds, in their gambols, prepare already a cradle for their progeny. What connexion is there between the impressions which captivate them, and the cares of their future maternity?

Before he can combine the new impressions which assail him in such a crowd, the infant has already its tastes, inclinations and desires; it employs all the feeble means in its power to manifest and satisfy them.

One thing still more worthy of remark, though perhaps it is noticed the least, is, all those passions which succeed each other so rapidly, and are pourtrayed with so much nature on the variable countenances of children. While the feeble muscles of their arms and legs are scarcely capable of executing some feeble movements, the muscles of the face already express, by distinct movements, though the elements of them are much more complicated than the former, almost the whole series of general affections or feelings peculiar to human nature; and the attentive observer readily recognizes in this picture, the characteristic features of the future man. Where seek the causes of so complicated an apprenticeship, of those habits composed of so many varied determinations? Where even find the principles of those passions which cannot be formed immediately; for they suppose the simultaneous and regular action of the whole sensitive organ? It cannot be surely in the impressions yet so recent, so confused and discordant, of external objects.

It is then, we may affirm, in the internal impressions, in their simultaneous concourse, in their sympathetic combinations, in their continued repetition during the whole time of gestation, that we must seek at once, both the source of those propensities which display themselves at the moment of birth, and of this language of physiognomy by which the child knows already how to express them, as also of the determinations which they produce.

Though we may not entirely coincide with the author in his notion of the source of these inclinations and affections, he has shown that they depend on a primitive organization.

Thus, he continues in another place, in animals in gene-

ral, and in man in particular, there are two very distinct kinds of impressions, which are the source of their ideas and of their moral determinations; and these two kinds are found but in different degrees in all species. For man, placed by some circumstances of his organization at the head of all animals, *participates in their instinctive faculties;* as in their turn, though deprived in a great part of the art of signs, which are the true means of comparing sensations, and of transforming them into thoughts, they participate to a certain extent in his intellectual faculties. And perhaps in looking a little more attentively, we shall find that the distance which separates him, under this last point of view, from certain species, is far less relatively to that which separates many of these same species one from another; and that the superiority of instinct which the greater number have over him, joined above all to their almost entire want of imagination, compensates in their real happiness for the advantages which have been lavished on him, and which they do not enjoy. Cabanis, *Rapports du Physique et du Moral de l'Homme. Tome I. Memoire* 2.

We come now to apply these Phrenological views to the study of the brain and craniology, which you must be sensible by this time is but a branch of Phrenology. Assuming these differences in the propensities and actions of animals as acknowledged and established, we naturally inquire whence it proceeds. Is it from the varied conformation of a tooth, a claw, or a hand? assuredly not; these are instruments for the gratification, but not the causes of instincts, which exist before the former can be exerted, as well as after they are destroyed, by disease, accident, or age.* Let us now recur to the original proposition advanc-

* "Man, it has been said, owes his intelligence to his hands; but the monkey, the declared enemy of all industry, has hands too. To ascribe the intelligence of man to his hands, is to deduce the cause from the means, and talent from the tool, with which it works. It is just as if I were to say that Le Sueur is indebted for the happy native graces of his pictures, to a pencil of sable's hair; and that Virgil owes all the harmony of his verses, to the feather of the swan of Mantua."—St. Pierre's *Studies of Nature.*

ed at the last meeting: "That the brain is the material instrument of mind, and the essential organ of animal life," without which neither instinct in animals, nor reason in man, can be displayed. To show that the brain is allotted for the faculties of the mind, and that even without it, the rest of the body may have all its parts healthy and entire, we shall give another extract from the works of Cabanis, already cited.

In order that there be, says this writer, integrity of the functions, it must exist in all the organs. It is particularly necessary that the cerebral system, and all its dependencies, meet with no lesion, either in their primitive formation itself, or subsequently, and by the effect of diseases. For example; to think, the brain must be healthy. The hydrocephalic, or those with dropsy of the brain, in whom its substance is destroyed and gradually effaced, become stupid. Still the influence of the spinal marrow is sufficient to give vitality to the viscera of the chest and abdomen; and even when this substance has undergone the fate of the brain, the large nervous trunks support for a long time the remains of life. Some children are born without head, (that is to say, without brain); they die some time after their birth, because nutrition, which was performed by the umbilical chord, can no longer take place in this manner, nor in any other sufficient for the support of life. But they are often in other respects large and plump; their limbs are well formed, and they have all the signs of strength.*

In other children, the state of the brain often entirely prevents thought. They do not live less healthy and vigorous; they digest well; all their other organs are well developed; and the instinctive determinations connected with human nature in general, are manifested by them very

* These facts receive additional elucidation by the observations of Professors Tiedeman and Gmelin, on the fœtus in the different periods of gestation; from which they are justified in asserting, that the spinal marrow, as necessary for the vitality and growth of the body, is first formed, and the brain, which is the organ for receiving external impressions and observing their relations, is of posterior formation.

nearly at the usual epochs and according to the usual laws. It is not long since I had an opportunity of observing one of these automatons. Its stupidity proceeded from the extreme smallness and bad conformation of the head, which never had any sutures. It was deaf from birth. *Though its eyes were in a good state, and appeared to receive some impressions from the light; it had no idea of distances.* It was however very healthy and strong; it ate with avidity. When they did not give it one piece very speedily after another, it threw itself into violent agitations. It delighted to handle what was presented to it, particularly animated bodies, the mild heat of which, and I believe also their emanations, appeared to be very agreeable to it.

To form a just idea of the operations of which thought is the result, we must consider the brain as a particular organ, specially destined to produce it; in the same manner as the stomach and intestines for operating digestion; the liver for secreting bile; the parotids, maxillary and sub-lingual glands, for preparing the salivary juices.—The impressions on arriving at the brain, make it enter into activity; as the aliments on passing into the stomach, excite it to the more abundant secretion of the gastric juice, and to the movements which accelerate their dissolution. The peculiar function of the one, is to perceive each particular impression, to attach signs to it, to combine the different impressions, to compare them together, to draw judgments from them and form determinations, as the function of the other is to act on the nutritive substances, the presence of which stimulates it to dissolve them, and to assimilate their juices to our nature.

From what precedes and from what we have already pointed out in the last memoir, continues the same author, we may readily conclude, that the nerves and the brain are not purely passive organs; that their functions suppose, on the contrary, a continual activity, which lasts as long as life. The nature of these functions, and the manner in which they are performed, would suffice to prove it. Besides the physiological knowledge of these organs, that is to say, the

knowledge of their structure and of the movements by which they are nourished, and reproduce without ceasing the immediate cause of sensibility, demonstrates it with an evidence that the eye may seize.*

Our first proposition now more fully confirmed and illustrated, we should, on the principle that varied effects, presume a variety of causes or complications of the one cause, infer *à priori*, that animals with different propensities should have brains differently modified or developed, and that in proportion as their instincts or innate propensities are more numerous and diversified, the structure of their brains should be more complex. Is this the case? It was first asserted and taught to be so by Gall and Spurzheim, and though denied by many as visionary and absurd, has now received the assent and entire concurrence of a Cuvier, a Blainville and a Geoffry St. Hilaire, names distinguished and cherished, wherever science, natural history and comparative anatomy, are studied or appreciated. These gentlemen, with a frankness, the characteristic of great minds, have acknowledged the benefits which such a discovery and arrangement give them, in the classification of animals, and in fixing their distinctive characters. The two last named, have avowed their conviction that the same method of observation may be extended to man, and that the situation of many of the cerebral or internal organs, with their corresponding manifestations, is proved beyond the possibility of doubt. But it is not in comparing different species of animals that we find such striking differences in the cerebral mass, but also in different individuals of the same species. Every one must be struck with the marked proportional difference in the size of the skull of a tiger and a dog, comparing only two similar regions or portions: but he will discover after a while, a great variety in the skulls of tigers, or dogs, or horses, and invariably will he find, on attentive

* The fibrous structure of the brain as displayed by Gall and Spurzheim, explains the active state of this organ and its periods of repose, like every other vital part.

and patient examination, that there is a corresponding variety of dispositions. "Some dogs, even of the same race, are surly, churlish and revengeful. Others are gay, frolicksome and friendly." It may be replied they all look alike; but put yourself in the path of observation and patient inquiry, and then note the result. If nature had not stamped on every individual bird, for example, a peculiar mark, it would be impossible that the numerous multitudes who pair or join in matrimony, should be capable of distinguishing and adhering faithfully to one another. A shepherd who has been long accustomed to superintend a numerous flock, knows by the countenances and other natural or accidental marks, every individual.

If we extend the range of observation from the lower animals up to man, we shall not be less forcibly struck with the distinctions drawn from cranial configuration, in the varieties of the human race. This is a pursuit alike interesting to the philosopher and the legislator, who may thereby respectively study human nature with more success, and enact laws adapted to situation and circumstance, rather than draw them from his own feelings and peculiar mode of thinking. We hope soon to be enabled, in imitation of one of the illustrious members of the Edinburgh Society, to present you with a series of casts of the varieties of mankind, and compare their cerebral development with character—you will then have before you the New Zealander, the Otaheitan, the Malay, the Gentoo, the Chinese, the Tartar, the Indian, the Carib, the Negro, and the Caucasian or European races: and have additional reason to be pleased with that study, which opens such a wide field of comparison, and leads us to an attentive observation of the whole human family. We shall then be gratified by much amusing speculation as well as practical application, in inquiring how far certain stocks are capable of improvement, and what benefits might result from education and good government. Basing our opinions on the physiology of the brain, and calling in the aid of history and actual observation, we shall, I am afraid, be compelled to dissent from the doctrine of Helvetius, and

acknowledge the same inequalities in the different races, that are seen in the individuals of any particular nation.* Taking up the head of a New Hollander, we must exclaim, in the language of Spurzheim, in vain may we expect a Locke or a Newton from millions such as these, even though they received all the lessons of the Academy, the lectures of an Aristotle, or the lights of modern science. In the rounded and narrow head of a Gentoo, we see the cause of their pertinacious adherence to superstitious forms, so frequently at variance with reason and justice, and of that timidity which makes them the slaves of every invader: and in the high conical head of the Carib, we read that pride and obstinacy for which they are so noted.

Would it be presumptuous to infer from differences like these, the cause of the inferiority of the ancient Egyptians to the Greeks in arts and sciences, notwithstanding their sixteen centuries of empire, and a crowded population, by which every stimulus was presented for useful invention or tasteful decoration. Compare the head of a Copt with a Greek, of an African with a European; look at the Sphinx, and the Apollo; and ascending from facts to causes, you will perhaps be induced to give your assent in part to the idea thus thrown out.

The two sexes present a difference in cerebral development, corresponding to that of their moral and intellectual manifestations. If man be more daring and intrepid, more inventive in expedients, and successful in higher combinations,—to woman, must be awarded greater circumspection and attachment; nor ought the former, proud of his stronger powers of reason, contemn as trivial that love of approbation so conspicuous in the latter, and which, when receiving a proper direction, and joined as it is in her to

* As we find avowed by Locke himself, who says, "There is, it is visible, a great variety in men's understandings, and their natural constitutions put so wide a difference between some men in this respect, that art and industry would never be able to master, and their very natures seem to want a foundation to raise on, that which other men easily attain unto." *Conduct of the Understanding.*

greater friendship and veneration, constitutes more than half the charm of life,—without which, " 'twere all a wilderness, a dream." It is these, which make her the ministering angel at the bed of pain, and feel rewarded by one smile of gratitude for days of anxious watching and deep sorrow. By her circumspection she eludes the shock, which, he proudly braving, too often sinks under; or if misfortune come, she draws consolation from above, and finds her happiness in soothing the partner of her grief, and winning a grateful look by her tender assiduities. This is not the dream of youth, but the realization of old age, which is only miserable when bemoaning

> ——————" The loss of her,
> That, like a jewel, has hung twenty years
> About his neck, yet never lost her lustre;
> Of her that *loved* him with that excellence
> That angels love good men with :"—

and proud ought we to be of that philosophy which can find confirmation strong, of truths that ought to be engraven on our minds as they have ever been wound around our hearts.

If we extend our view from animated nature, to those of her brightest, happiest imitations by the chisel of the sculptor, and pencil of the painter; and observe the numerous coincidences in favour of our science, we shall have additional reason to be pleased with a study which embraces such extent and variety of prospect, combines minuteness of detail with grandeur of combination, and imparts to the mind a habit of observation and analytical reasoning, very different from the cloudy and benumbing influence of the common metaphysical atmosphere.

It is a fact, not a little curious, that the ancient artists gave very generally to their gods and heroes, conformation of head corresponding with the present notions of Phrenology: thus, Jupiter, the father of gods and men, is represented with an uncommonly lofty forehead: Apollo and Hercules are made to differ from each other, not more in their forms, than in the relative size of their heads and propor-

tional development of these latter.—Every person must have noticed the contrast between the rectilinear forehead of the former, and the oxlike front of the latter; a longer observation would discover the posterior part of ther heads proportionally different.—A similar difference is observed in the heads of gladiators and philosophers,—the former showing the animal, the latter the reflecting man.—Let any one compare the head of an athleta or a gladiator with that of Socrates, or even a Chrysippus, to be satisfied of this fact. We may indeed be told, that these gods and philosophers were represented according to the taste and fancy of the artist; but in conceding this we have only additional proofs in our favour; for whence would the latter derive his models but from those most distinguished for the qualities which he wished to represent: and as Zeuxis painted his Helen from the most beautiful and lovely females of Crotona, so would the sculptor form his Jupiter and Apollo, from men the most distinguished for the extent of their understandings, loftiness of sentiment, or purity of taste. A remarkable exception to this general rule, is however presented in the Venus de Medicis, where the head, and particularly the forehead, is so disproportionately small as to be incompatible with any thing like the possession of common intellect. Was it ignorance in the artist, or did he intend to diminish our admiration for beauty by showing, that it alone was not sufficient without the all-inspiring mind, the living fount of the beauteous and sublime? We leave this as a new subject of enquiry for the Antiquary and Connoisseur.

The youthful physiognomist, in strolling through the Florentine Gallery, or the museums of the Vatican or Capitol, has doubtless amused himself in contemplating the busts of a Demosthenes or Cicero, a Cæsar or a Pompey, and trying to read in their countenances fervid eloquence, or noble daring.—But yielding him all the pleasure which youthful enthusiasm may derive from such a study, we might direct his attention to their heads, and point out to him more obvious differences, more marked variety of feature here than even the face presents; he will see the ide-

ality of Demosthenes, the causality, and strongly marked vanity of Cicero; or if he continue his views he will compare the head of a Nero and a Galba, with a Titus and an Antoninus, and see in the bust of the first when yet an infant, a sweet countenance truly, but that cerebral development which seemed to designate him for cruelty and tyranny, in despite of the moral lessons of a Seneca.

The modern artists have been less attentive to the head, than the ancient, but have very generally given to the foreheads, a configuration corresponding with the peculiar character of the personage represented. All the paintings of our Saviour and the Apostles, of the Fathers of the Church, and the Saints, exhibit a full rounded forehead, and an elevation in the centre of the upper ridge, corresponding to the organs of benevolence and veneration. Amid the innumerable illustrations, we may refer to the Redeemer and St. Peter and St. Paul, in Bologna, by Coreggio, and the Transfiguration by Raphael, who has given to the epileptic, or the man seized with a devil, not simply a peculiar physiognomonical expression, but also a configuration of head, showing deficiency of intellect, and forming a strong contrast to the figures on either side of our Saviour.—The Saint Cecilia of Caracci, and Saint Bruno, by many artists might be mentioned; but it is needless to dwell on this point, so happily displayed by all those whose genius and taste made them the delineators of nature. So well established is this correspondence between cranial feature and affective quality, that an artist who should present us a personage as a saint, with a low, flat forehead and head, however mild and expressive his physiognomy, appropriate his attitudes, or gracefully flowing his drapery, every spectator, from the unpractised peasant to the tutored connoisseur, would feel there was something wanting, and that the unction of manner which commands our respect and sympathy, was not to be found. We may *en passant* cite as a proof of the necessity of the painter's studying a certain harmony of proportion in the form and feature of the figures composing his group, as well as the rules of light and shade,

attitude and drapery, that a neglect of this study has made some of the best artists give us children with heads entirely disproportioned to their bodies and age.

Seeing then what a wide field is thus opened to our observation of animated nature, its varieties, its harmonies, from the stupid sloth up to man himself—from the most degraded being of the human race up to a Bacon and a Leibnitz—and in the exertions of art, from the misshapen image of wood to the perfection of the *beau ideal* in the Apollo Belvidere and a Niobe,—is it right or rational to insult our understandings by calling the study of Phrenology ridiculous and absurd? This same science enables us to class such men, to see the motives for their illiberality or ignorance, and derives additional confirmation of its truth from the unavailing efforts of opposition itself. We shall be enabled to distinguish the man of detail, from the one of higher powers of combination, and show that even an Aristarchus might criticise with success the poems of Homer, but be a very bad judge of the works of Aristotle. Each man has his gifts, and he should be cautious how he attempt what neither nature or education give him the power to accomplish, lest a rigid analysis show, that what was mistaken for intrinsic weight and splendour, is nothing but dross and artificial polish. Wit and ridicule may excite our smiles, or intimidate us from expressing our opinions, but can neither instruct nor convince; they may put a mask on truth, but are seldom able to drive away error. If we apply this general proposition to our study, we cannot indeed complain much of its having been retarded, in inquiring minds, by attempts at wit or satire; and the only criterion by which we should not wish the importance of the study to be measured, is the want of felicity with which these weapons have been wielded against us. When our science assumes more of the precision of the mathematics, and shall have the same rich and varied stores as abound in classic lore, we may then hope for Phrenology's occasionally affording subjects for unmixed amusement, similar to what we experience in the humorous essay on flying islands, or

Fig. 1.

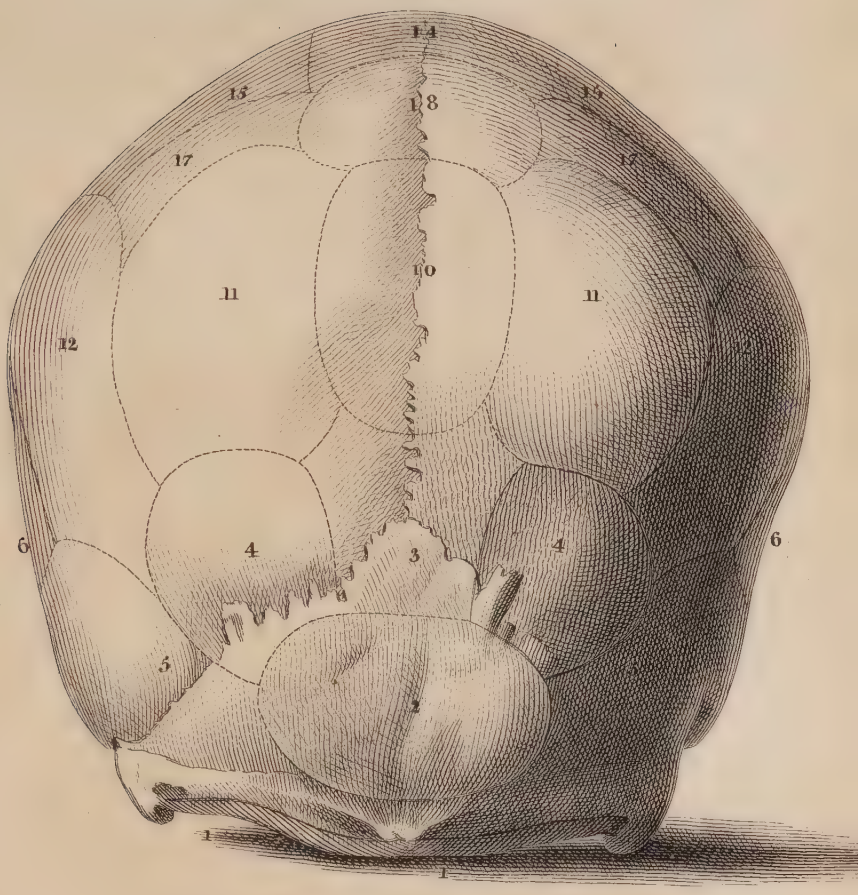
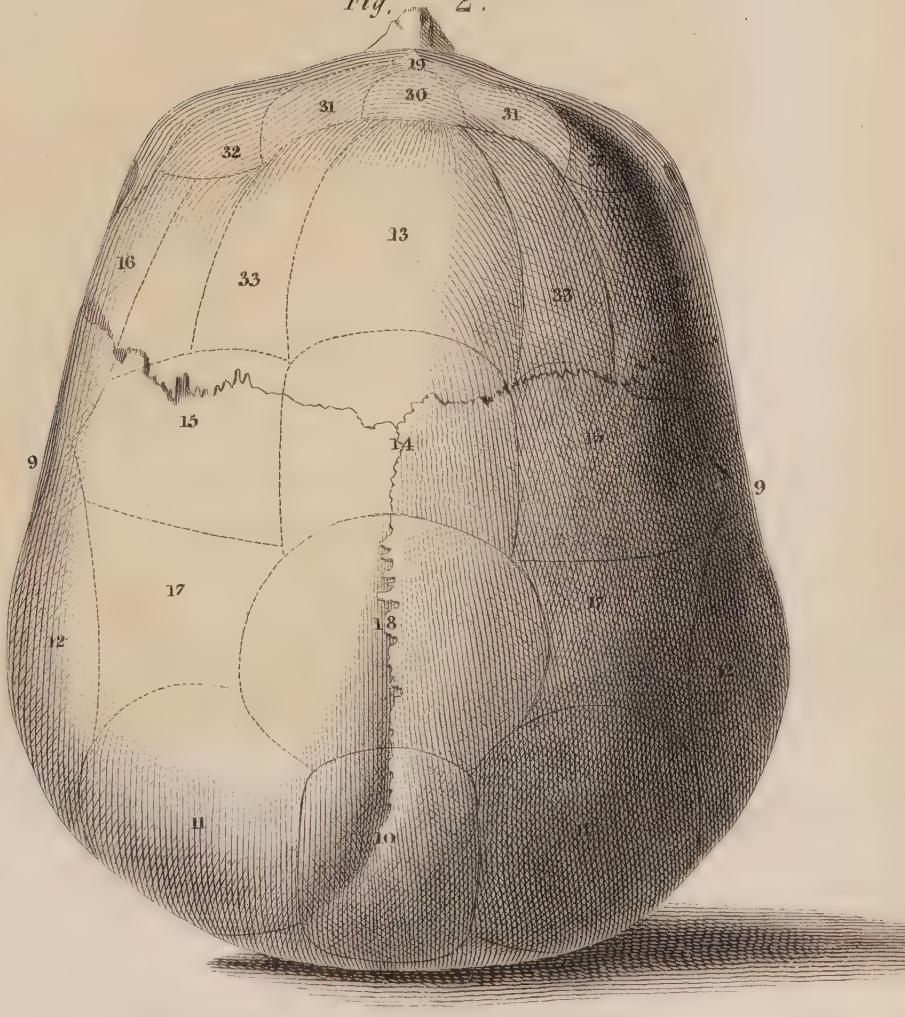


Fig. 2.



F. Kearny

Fig. 3.

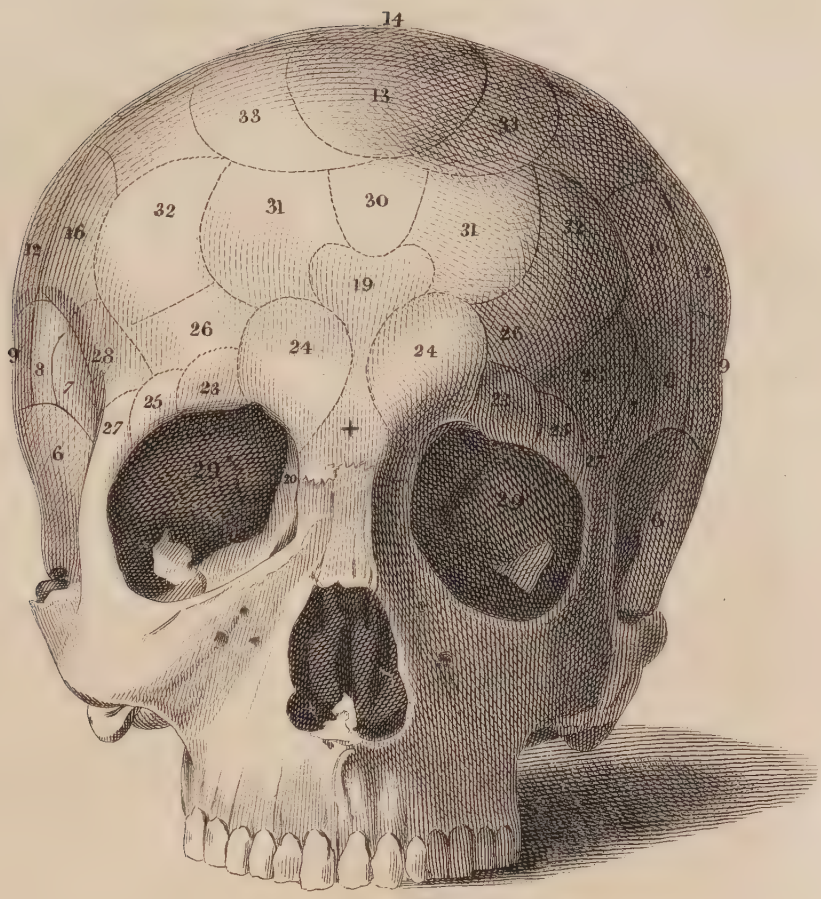
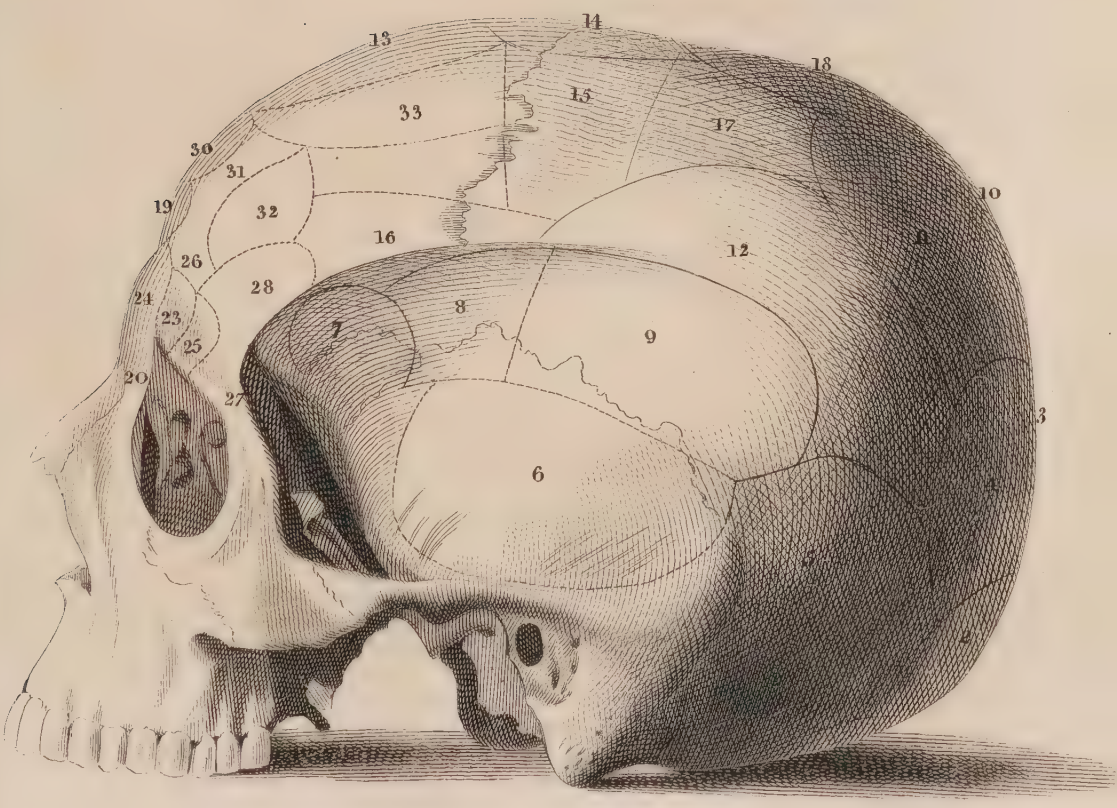


Fig. 4.



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the account of the philosopher with wings, and the learned disquisitions of a Parson Adams, or a Dominie Sampson. Swift and Johnson's ignorance, and consequent ridicule of natural philosophy, have not prevented us from benefiting by the discovery of steam-boats and telegraphs; nor will Fielding's and Scott's comic sketches render the classics less admired or instructive.

I shall not trespass on your patience any longer this evening, by a more particular notice of the organs as evidenced by the cranium; my object having been principally to direct your attention to the wide field before us, and to satisfy you, that our study is not simply skull-gaping or skull-gazing, craniology or cranioscopy, but truly and emphatically the study of mind, or Phrenology—in the prosecution of which, we follow the advice of Lord Bacon for studying natural history, which he divides into three parts: the species, or the kind; the wonderful, or monsters; the artificial, or that produced by man. In the species, we embrace the study of man and the various races; in the monstrous, we include congenital deficiency of brain, and injury by accident or disease, producing idiotism, partial extinction of intellect, and insanity; and in the artificial, we rank all the illustrations drawn from the fine arts and the sciences—ever holding in mind, that though genius may cast a meteor light over error, and a vivid imagination mislead for a while our better judgment, the truth must be finally determined by the maxim of the great man just quoted—*Quæ enim in Naturâ fundata sunt, crescunt et augentur: quæ autem in opinione variantur, non augentur. Nov. Organ. Lib. secund.* lxxiv.

ART. VII. *On Burns and Scalds.* By BENJAMIN H. COATES, M. D. *Read before the Philadelphia Academy of Medicine.*

IT may be supposed by many, on noticing this title, that not much of novelty can remain to be presented on such a subject—and that, whoever reads this paper will be obliged

to travel over a great mass of matter, which has become tedious from its familiarity. The following observations, however similar they may be to those which have occurred to many others, are such as have obtruded themselves upon the writer, rather in contradiction to established doctrines, and will be found to have much bearing upon the practice. Tame, and common-place, therefore, as this subject may be, he cannot help considering attention as well bestowed upon it, as on rarer and more singular objects of medical curiosity. It is well worthy of consideration, whether, among the throng of novelties, monthly and weekly offered to our view, we do not occasionally misapply time that would produce more profitable fruits if confined to maturing our thoughts upon diseases and remedies of more frequent importance. I am strongly impressed with the reflection, that if a very little improvement be made in the practice of treating fevers, pneumonic complaints, or any other of the large and copious outlets to human life, although it should only extend to saving one out of a thousand of those who die, it would be of more use to our species than if means were discovered of curing every case of hydrophobia that has occurred, or probably will occur, during the existence of this world.

Entertaining such views, I have prepared the ensuing observations on burns and scalds.

My conclusions have not been made without considerable opportunity of experience, and are the result of close observation of numerous cases, of which it would be easy to cite examples. Cases of burns, however, are so common and so completely within the power of surgeons, that it is thought needless to recount them—not wishing to abuse the reader's patience, by filling up sheets with such unnecessary and tedious details. I shall describe in general terms, the particular kinds of these injuries to which I allude, and refer to the reader's practical opportunities for a judgment of the fidelity of the descriptions, and the correctness of the practice—well aware that no modes of treatment can be correct or valuable, which will not abide this test.

There seems to be no slight degree of inattention in England and in this country, to the different extent and circumstances under which these accidents become the object of medical treatment. They are, generally, in a great measure lumped together, and the same general directions given for them all. One of those distinguished men of this city, who have gone to their repose, used to acknowledge, that the practice in burns was totally empirical. The application of cold, inculcated by Sir James Earle, had little opportunity to become generally introduced, when the prescriptions of Dr. Kentish superseded it, and hot turpentine became the reigning remedy. This was applied by many in a very indiscriminate manner, treating burns of all sizes and in all stages nearly in the same way. Variations were occasionally made by using alcohol, hartshorn, and some other articles—but the essential part of the practice was to stimulate in a violent degree. Of late, a new, and perhaps the best general prescription has been to envelop the part in raw cotton. To the great utility of this, I shall have future occasion to bear witness. If there be, as there is, a great variety in the nature and extent of the parts injured, and in the effect of the læsion on the system, there can be no doubt of the advantages of studying their variety and classifying them in practice.

Burns differ very greatly, indeed, in the nature of the organ affected, whether confined to the skin, or extending to a vital part. Of these by far the most common, and about which there is, at present, most room for discussion, are the cutaneous. The few remarks I shall offer upon the other class, will be, for the present, postponed.

When small burns occur on parts endued with little sensibility, as the outside of the limbs or the back, and but little irritation and no fever are produced by them, they may be simply wrapped in raw cotton so as to preserve them from chafing, and suffered to dry—or, where this is not likely to take place, a little ointment may be used beneath the cotton. It is very common to dip the cotton in linseed or other oil, as whale oil; but although the pre-

sent relief is quite as great by this as by any other mode, the dressing is not prevented from adhering to the edges of the ulceration or blister with so much force as to render its removal, for the purpose of change, difficult and painful. The only cerate or ointment which I have seen employed in this way, is basilicon, with or without the admixture of turpentine; and I am entirely convinced that simple cerate would answer perfectly well, as may, indeed, be inferred from the success of lamp oil. A weak digestive is useful, as we shall see, in the second stage, in order to promote a cure.

In small burns on sensitive and irritable parts, I would, unless forbid by constitutional delicacy or peculiarity, universally apply cold and moisture, by means of cold water, cold bread and water poultices, or scraped potatoes—and continue them until the pain is entirely removed, and for some time after. If the injury is on a projecting part, one of the most convenient and effectual modes of doing this, is to immerse it in a basin of cold water. As long as the cold is applied, the pain is, as far as I have ever learned, entirely removed; and indeed, it is difficult to name a remedy of which more than this can be said in any disease. There is obscurity and uncertainty enough in the practice of medicine and surgery, without rejecting a natural, safe, powerful and decided remedy which is already in our hands. A weak digestive should next be employed, under cotton or a similar substance, to promote the cure.

The principal practice of surgeons in burns is with those of a larger size, and which occasion irritation and otherwise interest the constitution, without at the same time inducing alarming depression, or threatening the immediate extinction of life. In cases of violence and extent beyond this description, there is generally, as we shall see, but little time afforded the surgeon to apply his remedies—and the most important part of practice is to treat properly the curable cases. It is to this place, therefore, that I have referred the principal points in dispute.

It is the duty of whoever handles this very common sub-

ject, to toil amidst discrepancies and embarrassments of the most irreconcilable sort, and to endeavour to strike out a rational practice from amid contending authorities, and the most contradictory modes of treatment, each asserted by its advocates to meet uniformly with the best success. Yet, I hope that it will not be impossible to show, that there is a decided preponderance of evidence in favour of that plan of treatment which corresponds the more nearly with the ordinary principles of pathology, and of a treatment nearly similar to that which is preferred by me.

The most natural inference respecting burns, and that which would probably suggest itself to the mind of one, who for the first time, and without preparatory instruction, beheld their phenomena, would seem to be, that they should be classed with other inflaming and disorganizing læsions, and that the treatment should correspond. This, with a few deviations, arising from peculiarities in the manner, degree and extent of application of the injuring cause, is probably nearer the truth, than among us is generally supposed. And at any rate, if we can approach, in some measure, to this simplicity in our results, it is a very desirable point gained, and must greatly facilitate the application of reason in our deductions, with respect both to general rules and particular cases.

From views of this kind, we should be led to employ the mildest emollient and repellent means in our power. Accordingly, such has been the practice in all ages, and in most countries of the world. The practice of the ancients will be found to correspond with this view, on a reference to Celsus.* It prevailed among the earlier surgeons of modern times, and is now in use among many English and American practitioners. In France, as appears from Petit and Boyer, as well as from private information, it is the prevailing treatment. It is also that of a very large and respectable portion of the surgeons of this country. There is, in short, evidence amply sufficient, of the general use

* Lib. 5. c. 27.

of these means among a great body of the most judicious men, and of its general competency, to enable those to stand at least on a footing of equality, who call in question the superiority and necessity of the unexampled practice which has lately been raised in opposition to them.

The use of cold, one of the prominent articles in a system at once emollient, astringent and repellent, is one of the commonest remedial means applied to these injuries in domestic life. Almost every child knows that, when he burns his finger, it continues easy as long as he keeps it immersed in a basin of water. From this heterodox and uncanonical cure, however, the kind mother or attendant generally prevents him, and the wiser boy, with distorted features, and probably screaming, is obliged to hold his hand to the fire and greatly aggravate the pain, in order that sympathy or attraction may "draw the fire out." Equally strange and unaccountable has been the conduct of some surgeons in prohibiting this simple and obvious remedy of nature. "Quid enim ratione timemus aut cupimus?" It may admit of a question, whether the use of cold as a remedy for inflammations in general, has received its just value in the minds of surgeons. This power, with those of heat and electricity, are perhaps the only agents which thoroughly penetrate the tissue of the living organs, and are often applied to the same fibres, whose modes or degrees of action they change. Accordingly, many very painful inflammations are greatly relieved, and often entirely cured, by this simple means. Its use in burns, the practice in the time of Benjamin Bell, is supported by perfect analogy, and the most entire consistency with daily experience. Abundance of evidence might be adduced from authors of its success in relieving pain and procuring a rapid recovery. Dr. Hartshorne has communicated to me a case, in which a comparative trial was made upon the two legs of the same patient, both which were scalded in the same manner and nearly to the same extent, one of them was placed in a vessel of cold water for half an hour, and then dressed with digestives, and treated in all respects, except the cold water, precisely like

the other. The leg first mentioned recovered considerably the sooner.

While engaged in trying and verifying the opinions of Sir James Earle, the medical world was astonished by the publications of Dr. Kentish, teaching a doctrine directly opposite. It was now to become fashionable to believe, that inflammation was to be abated by violent stimulants, and pain by the most tormenting applications known—conduct resembling that of those who enforce good principles and personal affection at the point of the bayonet. Originating with a village surgeon in the west of England, this doctrine was ordained to be confirmed by the corresponding opinions of professional men in various parts of that kingdom, and soon to be generally received as correct. I cannot help surmising, however, that these doctrines are not so generally entertained there as here, and here, as I observed above, they are not received universally.

Encouraged by the general success of so many surgeons, without the use of these means, and by a mode of treatment directly opposite, I have felt the more freedom in making public a dissent from the reports of its utility. It is said that no example is necessary, and that truth alone is a sufficient warrant for opposing any thing: but, although to a practitioner in medicine, a conviction of truth is, or ought to be sufficient motive for making up his own mind,—the case is radically different as regards his attempting to influence the minds of others. Unless some additional cause be given, besides the mere belief of an obscure individual, so various are the reports of medical practice, and so contradictory the results in different hands, that little weight is conceded them in the judgment of the reader. Hence the principal use of theoretical reasoning in essays, which else would be a mere incumbrance to physicians, whose powers are often much better qualified to create it for themselves.

I have had many opportunities of testing this practice, and followed, for a time, implicitly the directions given by the highest authority who were believers in the opinions of Kentish. It appeared to me, that when the natural irrita-

bility remained, the pain and suffering were always increased by the spirits of turpentine, and that in many instances, it could not be borne. We were told that in these instances the pain arose from the stimulant substance being applied to the " sound skin," and that the dressings should be cut exactly to the size of the burns. What was precisely meant by " sound skin," was not very clearly defined, though it was obvious that the application gave pain, when made to the inflamed skin, where the cuticle was not removed. I have applied it in every way, and with every precaution that could be suggested, both to the whole injured surface, including mere inflammation, to sloughs and blisters together, and to each of them separately. I have even cut the plasters *smaller* than the sores, so as in no instance to come at all in contact with the edges, and in every instance have found the same results, and that the pain varied only in proportion to the extent and violence alike of the injury and the remedy, and the irritability and sensibility of the patient.

The sufferer would, indeed, often confess that he felt ease, after a considerable interval, from each successive application of the turpentine, and supposing the doctor knew what was right, would attribute his relief to the dressing; and the acknowledgments of such patients have often been adduced to me as proofs of the advantages of the treatment. In the cases I saw, the pain always continued a sufficient length of time for the secretion of fluid, enough in quantity to coat the surface of the burn and protect it from the ointment. It appeared to me that relief was afforded sooner by any soothing dressing, with quiet. In one way it appears occasionally and perhaps not unfrequently to diminish the consciousness of pain, namely, by first increasing it to so severe and intolerable an amount that any less degree of suffering feels like relief. In this respect it has no inconsiderable resemblance to a popular story of an Indian, who when fatigued with a long journey, selects a heavy piece of wood, carries it for some distance, and then throws it down to " rest himself." From such relief it is desirable to save all we can.

The next object for which the turpentine is designed, is to reduce the sore into an healthy state. When the inflammation is removed, I can see no such peculiarity in burns as to require a different treatment from any other ulcer retaining that weakened state, so often the consequence of a preceding excitement, where the affected surface is coated with coagulated lymph, and disposed to be indolent in healing, from its powers of vitality having been exhausted by excessive exertion and distention of the vessels. In injuries of this kind, mildly stimulant dressings are, though indicated, not always indispensible. It is quite common, for instance, to see burns heal very tolerably under simple cerate. In confirmation of the propriety of treating these like any other indolent and debilitated ulcer, we may cite the practice of Dr. Physick, who has informed me, that he employs, and finds sufficiently stimulating for burns in this stage, the common ung. resinæ flavæ.

It may seem, however, presumptuous, to discard so utterly the practice recommended by Dr. Kentish, without attempting to give some explanation of the manner in which he was led to it. We may conceive, as mentioned above, the weight of authority on the other side sufficient to justify the exercise of perfect independence in forming a judgment—but it is no part of our argument to suppose that this gentleman is incapable of judging of the effects of remedies in ordinary cases, any more than to suppose that he would intentionally deceive the public. The greater part of his earlier cases were of that extensive sort of scalds, that produce extreme depression of the system at once, and render active stimulants of any kind adviseable, though frequently unavailing. He lived near a manufactory, where individuals frequently fell into hot liquids, and were scalded over great part of the body. In such cases as these, the practice alluded to, was doubtless useful, and probably the only correct one, and the endless differences of opinion, which occur among physicians on all subjects, are surely sufficient to authorize the supposition of his being deceived

in judging of other cases, by a preconceived theory. And when we add to this, that, at the time his publication was first made and began to obtain the most flattering kind of fame and distinction for the author, his medical education had been a partial one, he being only a village surgeon, it is not surprising, nor inconsistent with the general principles of human nature, that the physician of maturer years, more extensive studies and more enlarged experience, should over-value and apply too generally the discoveries of his youth.

In burns, then, of which the principal injury sustained by the constitution, is irritation and not depression, the first indication is to diminish that irritation by all the usual means. Two remedies particularly adapted to the nature of the injury, are cold water, and soft raw or carded cotton. The first is at once the most effectual of sedatives and astringents, and the most delightful of demulcents. If the injury is situated on a projecting part, the best mode of applying is by immersion—otherwise, wet cloths must be used. Regard will, of course, be had to the variety of circumstances which may render this improper, or may qualify or limit its application, such as the extent of surface demanding its application, idiosyncracy of the subject, or the weather. The application of a solution of lead is a long established practice. If poultices with cold water can be secured on the parts, they are among the best means to be next used. This, however, on these restless patients, is extremely difficult.

We come, then, next to the cotton. Of the modus operandi of this article I cannot find any evidence. It will probably be generally supposed to consist in the mere protection it affords from air and friction—the latter of which is no trifling consideration. How far wool, lint, or other similar matters may answer the same purpose, I cannot say. Cotton is an article so universally at hand, and of so well established utility, that in the cases, in which I have seen protection of this kind afforded, it has always been employ-

ed in preference to any thing else—attention being directed more to the immediate benefit of the patient, than to the elucidation of the therapeutical doctrine. Some mild unguent, as simple cerate or basilicon, should be applied beneath—as alluded to above, when speaking of more trivial burns. I have seen very good success from an ointment composed of simple cerate and powdered opium, in the proportion of a drachm of the latter to an ounce of the compound.

When burns reach their second or discharging state, I would dress them with basilicon, and treat them, in general, as common blisters and ulcers. I have used with advantage red precipitate, powdered chalk, carb. zinci impur.—and in some instances, even strips of adhesive plaster. The well known means now become requisite to avoid injurious or unsightly contractions, and ought, probably, always to succeed. Sloughs have separated best, in general, under a common poultice, occasionally changed for rye meal and yeast, or scraped carrots. Vesications, when not terminating in ulcers, it appeared to me, did best under simple cerate, or prepared chalk.

I have once tried, with very good results, the following prescription published by an empirical practitioner in the state of New-York. Take Burgundy pitch and wax, equal parts; sweet oil, enough to form a stiff cerate. Mix them. Burns should be disturbed, during their irritated and inflamed state, as little as possible; and it is desirable to let the first dressing remain for two days, unless necessity interferes. It is one of the disadvantages of the cotton, that it is very liable, under these circumstances, to produce a bad smell. On the second dressing, little difficulty will generally be found in deciding upon the proper mechanical defence necessary, as pledgits of cotton.

The usual remedies for the febrile and suffering state of the system, such as the lancet, purgatives, antimony, opium, &c. it is needless to particularize.

A very considerable danger now supervenes, in large

burns, and one to which authors have seldom, if at all, adverted, to wit: that arising from exhaustion, by the excessive discharge. Death has ensued at the end of several weeks, and long after the sores have commenced healing. A slow fever comes on resembling a hectic—the sores dry up, and the patients expire in the extremity of emaciation. Here every physician will see the propriety of doing all he can to support the enfeebled system. Opium, to procure sleep, is not to be neglected, and such mechanical means as prevent the irritation of the sores, and the fatigue of unnecessary motions, are undoubtedly valuable.

When a great part of the surface of the body is injured, and great depression ensues, when the pulse is small, weak, and often unfrequent, the mind is affected with stupor, or with typhoid delirium, and the heat of the surface diminished, and when the patient lies upon his back, uttering short, low, scarcely audible moans, and can with difficulty be induced to answer questions—then, there is full play for stimulants, and even of a more powerful kind than mere spirits of turpentine. I have seen the decoction of cantharides in the latter substance used, in several cases, and with a favourable effect. It was applied to the extremities, while Kentish's liniment was used on the trunk. But I am afraid, that when this state is brought on, and oil of turpentine becomes indicated, the number of recoveries, under any treatment, will be but small. " Hoc opus, hic labor est."

It has never occurred to me to see more than one case, in which evidence from dissection was afforded, that death was produced by the burn affecting a vital part. A man was admitted to the Pennsylvania Hospital, with a burn of no very great extent, occupying part of his breast and left arm—a few slight traces being visible on his face. It was not particularly observed, at first, that the edges of his lips and nostrils were scorched; and, as the surface visibly injured was not great, and the young man appeared vigorous and complained of no uncommon pain, nor of much distress in his lungs or other viscera, and as the pulse was, appa-

rently, rather less affected than was to be expected from the external injury. I fully expected a recovery. He died, however, without much warning, on the fourth day, and on opening the body, the trachea and bronchiæ, together with their ramifications, as far as we could trace them, were found quite black on their internal surface—thus evincing that, according to a popular doctrine, he had *breathed flame.*

CASES.

ART. VIII. *A Case of Suppression of the Menses, attended with unusual circumstances, relieved by the Volatile Tincture of Guaiacum.* By WILLIAM P. DEWEES, M. D.

ON the 6th of June, 1820, I was requested to see the daughter of Mr. D. F. who was represented to be suddenly taken extremely ill. I immediately visited her, and found her sitting up in bed, with her head tightly bound by an handkerchief. Her mother informed me, she was attacked with a severe pain about the region of the uterus, and almost instantly after with delirium or mental alienation. She complained much of her head, palpitation of the heart, and occasionally of a sense of suffocation. Her mental alienation was not constant; but, on the contrary, she had frequent lucid intervals, after a short continuance of the delirium, and in this latter condition, she would manifest extreme affright, begging protection from those around her, against some supposed impending and terrible danger.

Her situation previously to the severe attack just mentioned, was thus related by her mother. "She had not menstruated for seven or eight months;" (but it was afterwards determined, by the patient herself and sister, that she had not for more than a year) "that her belly swelled very much —her breasts became enlarged—she had nausea and vomitings in the mornings," &c. In a word, she had all the usual signs of pregnancy.

Examining the abdomen carefully, I found it considerably distended; there was a circumscribed tumour within it, which I was very certain was an enlarged uterus. While

conducting this examination, I thought I distinctly perceived the motion of a fœtus.

I merely prescribed for the more immediate and pressing symptoms. As there was fever, she was ordered to be blooded, purged, and to observe an abstemious diet. By these means she was in a few days, apparently, perfectly well, and so remained for nearly a fortnight. At the end of this time she was again attacked with precisely the same symptoms as just described, and which were subdued by the same remedies. These going off she again enjoyed an interval of two or three weeks of health, and was then seized as before, and a third time relieved by the same routine.

The abdomen continued to augment in size, and the feet and ankles were also swoln. I again carefully examined the abdomen, and now found an evident fluctuation. I had formerly thought I had perceived this, but it was obscure, and as I was disposed, in spite of myself, to suspect there was a gravid uterus, and imagined I might have been mistaken with respect to the fluctuation, though the young lady's character was, from the respectability of her connexions, and the propriety of her own conduct, beyond suspicion. It was, however, no longer doubtful, that there was water within the cavity of the abdomen, and also, that the uterus, from some cause or other, was much distended. I began to reflect seriously upon the case, and to weigh with great caution, all the circumstances connected with it. The young lady's character was without reproach—she had now been obstructed more than thirteen months—there was no perceptible increase of size in the uterus since I first examined her, though nearly two months had elapsed. Had she been pregnant at that time, she should now have been delivered, for the fundus of the uterus, if it was the uterus I first felt, indicated a period beyond the seventh month, for it was half way between the umbilicus and the scrobiculus cordis. When I reflected upon all these circumstances, I thought the complaint should be treated as ascites, and I therefore prescribed the volatile tincture of guaiacum, in doses of a tea-spoonful every morning, noon and evening, in a

wine-glass full of milk.* After taking this medicine for a few days, it purged her very briskly, and made her discharge very large quantities of urine. The tincture thus affecting her bowels, a few drops of laudanum were added to each dose of it, which enabled her to persevere in its use for three weeks without further inconvenience: at the end of this time, not a vestige of water was discoverable in the abdomen, but a serous discharge was observed from the vagina, which was soon followed by a sudden gush of fluid blood, to the amount of about three pints, which soon abated in quantity, and at the end of a week entirely ceased. She menstruated at regular periods after this, and now enjoys perfectly good health.

The above case is remarkable for the condition of the uterus, and the dropsy which accompanied it. This organ it would seem, did not refuse its habitual office, but that from some cause or other, the os uteri became closed so as to prevent the escape of the menstruous fluid. How this was effected is left to conjecture. May not the secretion natural to this part have become vitiated, so as to have its tenacity much increased, and thus block up the uterine outlet? That it was owing to some such cause is rendered probable, by the change just mentioned, namely, of the return of the ordinary secretion of the parts, in more considerable quantity than usual, which, after continuing a short time, was followed by a gush of fluid blood from the uterus.

Two causes might operate to the relief in this case. The natural secretion being restored, would of course diminish the force with which the obstructing cause might adhere to

* It may not be amiss to observe, that the preparation of Guaiacum which I employ, differs from that commonly obtained from the shops. I shall therefore subjoin my formula.

R. Pulv. Gum. Guaiac.	eight ounces,
Carbon. Sod. vel potas.	three drachms,
Pulv. piment.	two ounces,
Alcohol. dilut.	two pounds.

Digest for two or three days.

The volatile spirit of ammonia is added pro re nata, in the proportion of one drachm to every four ounces of the tincture.

the sides of the neck of the uterus, or the uterus being much distended, would, as in common pregnancy, begin to react, and this with sufficient force to overcome the resistance offered at the orifice of the uterus, now perhaps weakened by the restoration of the natural and healthy powers of this part. This conjecture is rendered more than probable, by the very sudden way in which the relief was obtained. At all events, it could not have arisen from any deranged condition of structure in the neck of the uterus, as this most probably, would not have admitted of cure.

This case is also interesting in other points of view. It marks with great force, the sympathies depending upon the uterus, when its cavity is in any way occupied; for in the case just related, all the rational signs of pregnancy were present, without impregnation—and to excite the sympathies, it would seem to be only necessary to have the parietes of the uterus put upon the stretch: and again, that the secretion of the menstruous fluid will not interrupt the play of sympathies between the womb, stomach and breasts, provided there be any cause resident in the uterus capable of distending its sides. It also proves the decided efficacy of the Tinct. Guaiac. in removing dropsy and amenorrhœa. In a moral point of view, it is not less interesting, as it shows how easily a derangement in the uterine economy, may lead to the most cruel and unjust imputations, even to affect life itself. Of the latter kind, Foderé* gives us an affecting account. "A young woman had her menses suddenly suppressed in consequence of a fright, and sought every aid to restore them, without effect: she was at length married, with a view to provoke a return, which succeeded after a time, and she discharged a great quantity of fetid matters. This fact was proved by the husband and the medical attendants. It so happened just at this period, that two children were found exposed and destroyed by cold—a suspicion fell upon this young woman because she was known to have had an enlarged abdomen, which very

* Med. Legal. vol. i. p. 476.

suddenly had subsided. The judges of the district, not free from prejudice against her, appointed a month after, a physician, a surgeon, and two midwives, to examine her, and determine, whether she was delivered of a child, or relieved of a dropsy of the uterus—who reported, " that they had discovered marks of delivery." In consequence of this, the poor woman was condemned to death, for concealing her pregnancy, and her children. An appeal however was made to parliament—and in consequence of two consultations, held by several physicians and surgeons of the greatest eminence, she was acquitted."

ART. IX. *Cases of Varioloid Disease. Communicated in a Letter to Professor Coxe, from* JAMES NORCOM, M. D.

I HAVE been much astonished by the accounts lately published in our country, of the varioloid epidemic, which has appeared in Scotland and in other parts of Europe, in the course of the last seven or eight years; and although I do not confide in every thing asserted about it, being naturally incredulous and distrustful in such cases, they have reminded me of a very singular case that fell under my own observation. About four or five years ago, I was called upon by the commissioners of navigation here, to visit a vessel at quarantine, twelve or thirteen days from New-York, and reputed to have the small-pox on board. Five or six days after the vessel left New-York, a man on board was seized with small-pox, the eruption appearing two or three days afterwards, and the man dying about four days before the schooner anchored in our port. Of the crew, consisting of five other persons, only one had had the small-pox, and all had been equally exposed to the infection. I had not the smallest doubt as to their all being infected with variolous matter, but having in my pocket some recent vaccine virus, and not having met with such a case before, I determined

to try the effect of introducing it. Three men and a boy were accordingly vaccinated. In one case, the course of the inflammation appeared to be regular, distinctive, and characteristic of vaccine, till the sixth or seventh day, at which time the vesicle appeared to have nearly reached its maturity, exhibiting the vesicular ring filled with limpid matter, and surrounding a central depression. Immediately afterwards, and before the efflorescence, which usually occurs about the tenth day from vaccination, made its appearance, the part assumed a pustular form, its contents became obviously puriform, and on the ninth day, when the symptomatic fever of small-pox came on, it was to all intents and purposes a variolous pustule, (i. e.) exhibited the same appearance as a pustule produced by inoculation for small-pox. The efflorescence followed on the tenth or eleventh day, but was rather indistinct, and the fever was followed by a small crop of pustules, which in seven or eight days came to maturity, and in thirteen scabbed off.

The other cases were less regular and distinct at first, two of them being blacks—but they all invariably put on, for the first five or six days, most of the appearances of vaccination, yet all presenting at the period of their maturity, a well characterized, distinct, prominent pustule. They all had what I shall call the small-pox mildly, and all recovered. Do you believe, from all you have collected or experienced on the subject, that these people can have the small-pox again? Would not the matter, in all the cases, when at maturity, have produced small-pox? What would have been the result of inserting the matter from the first patient on the sixth day after vaccination? I do not believe that the vaccination in either case reproduced the genuine virus of kine-pox! It certainly, or *at least probably*, lessened the violence of the variolous power, and gave mildness to its effects—but the character of small-pox was distinctly and plainly legible in all of them, and its course and duration natural and uninterrupted.

I still believe in the importance and efficacy of vaccination as a preventive of small-pox, when the business is

skilfully managed; and so far from deprecating, with you, the interference of government in the case, I am decidedly of opinion that an interposition of its power, guided by the experience and skill of the faculty, is the only means that can save us from a most awful calamity. Vaccination, in my opinion, ought to be confined by civil authority to the hands of skilful and experienced persons, to be paid by the government; or it ought to be prohibited altogether. I do not mean by this to assert, that the intelligent physicians of the country are not fully adequate to the performance of this great work, could they pursue the plans which integrity and wisdom would suggest. But I think it is seriously to be apprehended, that against the prejudice, the suspicions, the ignorance, the dissentions, the hostility and the opposition with which they would have to contend, they could not be expected to make an effectual progress. These might all be put down by the arm of civil power, and measures might be taken to enforce a course of proceeding that would secure to our citizens a state of permanent safety, and eradicate from our soil and our cities this destructive and loathsome plague. I do not believe, with Dr. Jenner, that a sore ear or a single serous blotch on the skin, can so affect the progress of vaccination, as that it shall be ineffectual or abortive, unless that blotch or excoriation be connected with a state of general morbid excitement, of such a degree or character, as to supersede the constitutional impression of kine-pox entirely—and I do believe, contrary to the opinion of Dr. Potter of Baltimore, that all physicians of skill, who have given proper attention to this matter, have a criterion, by which they may pronounce with great assurance, that vaccination has been effectual. I do not know what is meant by Dr. Waterhouse's crusts—but I am clearly of opinion, that the scab cannot be relied on for genuine vaccine matter, except in cases where the vesicle has not been punctured, and has continued unbroken from the time of its formation. Yet I cannot conceive that the matter should be

good from the seventh to the ninth day, bad from the tenth to the fourteenth, and afterwards recover its distinctive character, so as to be at last unexceptionable: I mean in cases where the vesicle has not been broken or punctured at all.

ART. X. *Cases illustrative of the Efficacy of Blisters over the Eye in Ophthalmia.* By P. W. LITTLE, M. D.

PERHAPS few diseases have stronger claims on the attention and sympathy of physicians, than ophthalmia. The intense heat, pain, intolerance of light, turgescence of the blood-vessels, and tension of the parts, render the patient miserably uncomfortable, and deprive him of all the pleasure derived from vision, as well as prevent him from attending to the necessary and ordinary transactions of life. To remove the complaints of the eye as speedily as possible, and thereby prevent the ravages of disease, and the concomitant evils resulting therefrom, is also a consideration which ought to influence us in the choice and application of remedies. Having frequently experienced chagrin and disappointment in consequence of the treatment usually directed by our best authors, proving ineffectual, I was led to investigate the nature of the diseases of the eye with more particular attention, and to the selection of such remedies as promised to be most promptly serviceable. Considering how beneficial blisters are in removing inflammatory action, when put directly over the part affected, and that if applied to cover the eye might prove equally serviceable, by the abstraction of fluids from the vessels immediately interested, I was induced to have recourse to them in this way in a violent case of ophthalmia, that occurred in my practice in 1815. In this case, my hopes of the good effects of the remedy were fully realized, and that so quickly, that I was from that time determined to have recourse to it in this way whenever I should find it necessary to blister in that complaint.

In 1817 another case occurred, that of a negro child aged five years. The inflammation, turgescence of the blood-vessels and swelling of the parts, were so considerable, that her eyes had been entirely closed for many days previous to my seeing her. There was also a considerable purulent discharge, and so acrid as to excoriate her cheeks. The complaint had been of eighteen months standing, and a variety of remedies, such as collyria, purging, bleeding, blistering the temples, &c. had been used without any effect. I bled her copiously, gave a mercurial cathartic, and applied a blister which covered both eyes, and extended for some distance on the forehead and cheeks, interposing a piece of gauze between the blister and eyes—a small compress was then laid on, to fill the cavity, and a second, the size of the blister, and the whole confined by a roller, and directed to be left on seven hours—then to be dressed three times a day, with cabbage or plantain leaves. Previous to applying the blister, the parts were rubbed wet with warm vinegar. As she had fever, nitre and emetic tartar were given, in nauseating doses, at stated intervals. The blister rose well, and discharged very copiously, and relieved her eyes speedily from the pain and inflammation; but a weakness and partial opacity of the cornea remained, to remove which an eye water, consisting of a solution of sugar of lead and white vitriol, in rose water, with the addition of laudanum, was directed to be occasionally used. The opacity of the cornea and erysipelatous inflammation that affected the ciliæ, were removed by mercurial ointment. As the disease had been of long standing, and manifested a disposition to return, blisters were directed to be applied to the temples, and afterwards to the nape of the neck, hoping thereby to produce a revulsion, which with the occasional use of blood-letting and mercurial purgatives, entirely relieved my patient. I have, since 1817, had repeated opportunities of using blisters in cases of ophthalmia, particularly during the last autumn and the present winter, the disease being epidemic. But it was not characterized by any peculiarity of symptoms, other than a most excruciating pain, seated

above the orbit of the eye—a great extravasation of red blood, which gave to the sclerotica and conjunctiva an appearance resembling, to use their own expression, *raw beef*, attended with intolerable itchiness and a profuse watery discharge, particularly in the fore part of the day. But as the day advanced, this discharge ceased, and the pain and intense heat which followed were almost intolerable. This affection of the eye was preceded by shivering, and attended with evident exacerbations coming on regularly every evening. The biliary system was much deranged. Mercurial purgatives very generally brought away highly bilious stools, varying in their appearance from a pale green to a black colour, and of the consistence of tar. The usual saturnine colyria proved injurious, even in a diluted state, unless when combined with laudanum, which I frequently did with advantage in the milder cases. Mercurial ointment proved also serviceable in such cases, by allaying the itchiness that always existed, in every grade and stage of the disease. Those who escaped the prevailing fever, were more generally the subjects of ophthalmia, and from the shiverings and chills which preceded the exacerbations that regularly attended, together with the biliary derangement, manifested that the prevailing general disease, had in those cases *located* itself in the eye. Copious blood-letting generally before the application of the epispastic, which, followed by mercurial and saline purgatives, and such other medicines as the state of the system demanded, constituted the treatment.

Deeming it unnecessary to relate in detail the numerous cases of ophthalmia, in which I have found a blister so much superior to every other measure in removing pain and inflammation, when applied over the eye, I shall content myself by only further observing, that a violent case of the disease occurring in a child of only five months old, was entirely relieved by this application. Her eyes were in the highest possible state of inflammation, and so turgid and excoriated for some distance around, that they had not been opened for several days, and the restoration of

sight was entirely despaired of. Every remedy, except the blister usually employed in such cases, had been tried in vain. The blister was now applied over both eyes, and the excoriated parts, and was the happy means of speedily allaying the inflammation, restoring sight, and preventing all unpleasant consequences.

It is true, a blister appears to be a harsh remedy, when thus applied to the eyes of so young an infant. But its effects were so promptly beneficial in this, as well as in all the other cases in which it was used, that within the circle of my practice, it is now called for by those who suffer much from ophthalmia, or any other violent affection of the eye. In persons subject to habitual sore eyes, of which there are many cases, blisters in the same way, may be occasionally applied—and afterwards a revulsive action produced by them, on the temples or nape of the neck, or between the shoulders, will be found a most invaluable remedy, particularly when aided by alterative medicines.

In staphyloma I should also expect to derive much advantage from a blister, as above directed, as well as in incipient cataract when aided by internal remedies.

Cases are on record, where cataract has been dispersed by casual inflammation—and is not the inflammation produced by the blister, in addition to the continued discharge for several days from the parts in immediate connexion with the eye, well calculated to remove internal morbid derangement in the early stage. It is proper to observe, that in every case in which a blister was applied over the eye, that the pain, however excruciating, was relieved as soon as it began to draw, and the inflammation and redness of the eye were removed before the blister healed—and that weakness of the eye, which so generally follows violent inflammation, was entirely prevented.

REVIEWS.

ART. XI. *Observations sur la Fièvre Jaune, faites à Cadix en 1819, par MM. Pariset et Mazet, Docteurs en Médecine de la Faculté de Paris, et redigées par M. Pariset in 4to, Paris, 1820.**

IT is a common saying, that he who wishes to prove too much, proves nothing. Never did the truth of these few words manifest itself to our minds more forcibly than in the perusal of the work now before us.

It is, perhaps, sufficiently known to many of our readers, that the French government, animated by the laudable desire of promoting the advancement of the study of epidemical diseases, has, at different periods, sent physicians to infected districts, even out of its own dominions, for the express purpose of investigating the nature of the reigning pestilence, of aiding by their learning the practitioners of the country in devising an appropriate plan of treatment for its cure, and likewise, the most effectual means of preventing its recurrence. In this way, the diseases which have ravaged Germany, Italy, Spain, received some elucidation; and if the information thus derived, be not thought commensurate with the very high expectations which it was natural to form, the blame is surely not to be attached to the authority that planned, but to the incapacity of those who were intrusted with the execution of these eminently meritorious projects.

Among the diseases to which attention was naturally drawn, the yellow fever stands conspicuous. This malady, which for the last thirty years has continued to be the annual scourge of some of the fairest portions of our country, has, in like manner, committed great havoc in Italy, and

* Communicated.

still more in Spain, where, in the year 1800, it caused, in the province of Andalusia alone, the loss of nearly eighty thousand lives.

The work, of which we now purpose to give our readers some account, has been sent over by the French government to this country with great pomp, and presented to some of our learned societies, with a public invitation, addressed to physicians and other scientific men, to come and peruse it. The author of it is already advantageously known, as a good scholar and an able writer. He has translated into French some of the works of Hippocrates, written some of the articles in the Dictionnaire des Sciences Médicales, and furnished some for the different Medical Journals of France, of no ordinary value. Coming with such pretensions, we were exceedingly anxiously to examine this work. But the pleasure and instruction we anticipated receiving, have not in the least degree been realized. Instead of finding it to contain an impartial and fair detail of facts, arranged and presented with all the systematic order generally observed in French scientific writings, we have discovered a mere mass of confusion—made up of declarations and inconsistencies—of unphilosophical conclusions drawn from ill digested data—of vague notions of contagion, and infection, of which he does not seem to understand the most simple laws, and of the opinions of some Spanish physicians, whom he continually cites as illustrious, learned, &c. In the whole course of the work, we have, in fact, found nothing which can in the least contribute to the advancement of the science, or to the establishment of his reputation, or from which the inquiring mind can derive one single atom of information. These are charges the most heavy and serious, and by the generality of our readers may perhaps be considered as too peremptorily preferred—But we have thought it almost indispensable, thus to give our opinion of a work, in which the most preposterous notions are presented in a most fascinating style, but with a tone of decision and arrogance too often practiced by those the least acquainted with the subject of which they treat.

In proof of the correctness of these animadversions, we need not proceed far into the work. The following passage, extracted from the second page of the preface, will sufficiently vindicate us. It serves, likewise, as an admirable specimen of the author's mode of reasoning, and will, moreover, expose some of the views he entertains on one of the most interesting questions, which forms at the present time a subject of difference among the members of the medical community. It relates to the contagious nature of the disease.

"Now, I do not believe that the contagiousness of any fever has ever been more decidedly proved, than that of the yellow fever of Andalusia. However favourable to the contrary opinion, the testimonies of the most distinguished physicians, those of Drs. Deveze and Valentin more particularly, may be, their authority can have weight with respect only to those facts they have observed themselves, and not to all the rest—since what proves true in the west, is perhaps not so in the east. The second point relates to the origin of the yellow fever of Europe. Does this disease originate spontaneously in Spain, or is it conveyed thither from some foreign source? Placed between these two suppositions, and notwithstanding the many subtle arguments made use of to establish the first, and by which I had myself been seduced, I do not hesitate now to decide in favour of the second. It is founded on the strongest presumptions, and I do not fear to advance it, if not as a positive truth, at least as a very probable one. But from whence does Spain derive the yellow fever? Most generally it is brought from America—but the epidemic of 1819 came in all probability from the East Indies.

"I regret very much that the disease which has ravaged for a long time the coast of Coromandel, from Pondicherry to Calcutta, is not more accurately understood, since it is hitherto only known by a very equivocal qualification. From notes in my possession, which however are very imperfect, this disease appears not contagious: but if it is the same that has desolated the isle of France, we shall be compelled to form an opinion of it totally different. From more recent information on the subject, it appears, that the disease of the Isle of France was propagated in the same manner as the yellow fever of Andalusia. Now, if the latter was contagious, why deny this quality in the first? We find, moreover, great analogy in their symptoms. Why refuse to believe in their identity? But the yellow fever of Cadiz came from Calcutta. That of the Isle of France was carried thither from Manilla by an English frigate. Which of

these places is the primary source of the disease? Is it Manilla? Is it Calcutta? Or are there at the same time two sources of infection?"

But this is not all. The author has said that the winter of 1819, 20, had probably destroyed all the germs of the yellow fever in Andalusia, and that unless re-imported, it would not re-appear in that province. The fever, he now tells us, again made its appearance there last year—though for the *honour of his prediction*, he attributes it to a cause, which, *though real*, will, he fears, startle sceptic minds, namely, to the power possessed by contagious germs of being retained in the system even for a great length of time. To this cause he also attributes the appearance of the fever, which he *modestly confesses* broke out last year, in two small habitations of the Sierra Morena. Since it is *probable*, some of the inhabitants of these places had visited Xeres or Seville, during the time of the epidemic of the year before, and then derived the fatal germs which were only developed twelve months subsequently. This, he tells us, is an hypothesis well worthy of investigation. As to marshes or other sources of deleterious effluvia in the neighbourhood of the Sierra Morena, he says, "if the disease owed its origin to these, why did it not appear the preceding years?" but he goes on to maintain, that their existence, owing to the very elevated situation of the Sierra, is not at all *probable*, and consequently, other causes (than above mentioned) must be sought, to account for the appearance of the fever.

This, it will be confessed, is not the way to write the history of epidemical diseases. But we have given these passages merely for the purpose of placing before our readers a specimen of Dr. Pariset's system of *probabilities* and *improbabilities*, and an example of his mode of reasoning, and therefore, without further comment, we proceed to notice the work itself.

To follow our author from Paris to Cadiz, and to detail all the minute circumstances and interesting trifles of his

journey, would prove equally unnecessary and fatiguing. It may suffice to state, that on the 26th of October 1819, he received a note from the Duke Decazes, inviting him to undertake the journey—to which he readily assented, and on the 3d of November left Paris in company with Dr. Mazet who had been appointed his colleague. On the 18th of the same month, they reached Madrid—left there on the 23d, and arrived at Seville on the 28th. But here they were too late. The fever which desolated this city, had already ceased, and the account of its symptoms with which he has favoured us, is merely a transcript from that given by the physicians of the place. He especially quotes Dr. Velasquez, but as from the description, it would appear the fever perfectly corresponded with that of this country, and as autopsic examinations have revealed the same morbid phenomena, we abstain from citing any part of it. After remarking that the physicians of Seville were a long time ere they could be convinced of the identity of their pestilence with the fever of the Antilles, the disease of Siam, of Philadelphia, or Havannah, he goes on to say, that the identity was but *apparent*, the yellow fever of the Antilles being *never contagious*, that of Seville *eminently so.*

"All the members of the medical commission, (of Seville) without one single exception, are of this opinion. The memoir, from which I have given an extract (Velasquez's) is almost wholly founded on the supposed existence of a *contagium*—and until all the facts authorizing this belief are refuted, or until it shall be proved, that of two fevers held to be of the same nature, the one may be contagious, and the other not in the least so, without however affecting their identity, I maintain, that it is dangerous to confound them, one with the other, as well as to restrict them to the same police and same mode of treatment."

We would beg our readers to bear in mind this declared opinion of Dr. Pariset, and to compare it with that passage, we have extracted from his preface, where he affirms, that most generally, the yellow fever is derived from America. If this latter opinion be correct, the fever must be contagious and identical with that of Spain, and now in refu-

tation of his own assertion, he gravely tells us that the disease, as it appears in the Antilles, is never possessed of this quality. But this is not the greatest of the numerous inconsistencies with which his work abounds, as we shall have occasion to point out in the progress of this article.

Having stated that the epidemic had first appeared in a quarter of the city called St. Crux, and that some believed it to have been caused by the exhumation of dead bodies, some that it proceeded from certain provisions brought from Cadiz, and others again, that it had been carried by a woman from San Fernando, he proceeds to give a description of that part of Seville.

"I was struck," he says, "with the singular structure of this quarter. It is composed of a mass of houses of little height, white-washed both outside and inside, as in all other parts of Andalusia—but separated by crooked streets, and so narrow, as not to admit of the passage of two persons abreast—even, in some parts, I touched at the same time with my elbows the houses on both sides. These narrow streets are paved with small stones, which being easily detached, form holes in which the filth accumulates. It is easy to conceive, that in streets so narrow, tortuous and irregular, and which had already excited my surprise in several cities of Spain, though more particularly in Cordova, the air is very rarely changed, and that during the long state of stagnation, it becomes *saturated with all the vapours exhaled from every kind of substances.* Add to this, that during the heat of the summer, the inhabitants, for the purpose of intercepting the light and abating the ardour of the sun, commonly spread tents from one house to the other. Thus sheltered, they are likewise immersed in their own emanations, and, should the inhabitants remain confined in dwellings so disposed, these animal exhalations, augmented by high temperature, and accumulating in a still atmosphere already charged with other vapours, must render the habitations not only capable of receiving and propagating, but likewise of *producing spontaneously* the mos tmortal diseases, as is observed in vessels, camps, prisons, hospitals."—P. 22.

Now it appears to us, and in which we are convinced most of our readers will concur, that after the description of this filthy place, and the declaration of our author, that all these exhalations could, in Seville, not only serve to propagate, but also to *engender* the most mortal diseases, he

would, extending the idea a little further, have confessed their capability of producing in that place, as they do in the West Indies, in other parts of the world, and also sometimes in ships, &c. the yellow fever, and that from this cause, independently of any other, had originated the pestilence in Seville. But in this we were sadly disappointed. Entertaining strongly the opinion, that in Spain, the disease must owe its origin to contagion, he neglects the most obvious of all causes, which a prejudiced mind only would have done, and after torturing his imagination, seems to incline to the belief, that to a woman from the plains of Chiclana the introduction of the disease was to be attributed.

From the report of the magistrate, Don Marino, it appears that the number of cases amounted to twelve thousand, and that of the deaths to one thousand five hundred—a proportion very small, when compared to the mortality among us from the same disease.

"However attentive we were to multiply our visits at the hospital, it will be evident to every one, that had the number of cases we had an opportunity of examining, been ten times greater, our experience of an epidemic so diversified in its nature, would still have been too imperfect. For this reason, we were compelled to supply our want of personal experience by that of our friends of Cadiz, and with the communications which they had the goodness to make us, and of which we have profited."

He, therefore, proceeds to give an account of the symptoms, from a work written by Don C. Francisco Rodriguez. But these being similar to those observed at Seville, and the treatment pursued by the Spanish physicians, offering nothing worthy of notice, except it be a decided aversion to the use of the lancet, notwithstanding the *strong frequent hard pulse*, constantly existing in the inflammatory stages of the disease, we shall pass them by, and enter on a part of our subject the most interesting—and from the inconsistencies, we were inclined to say, absurdities of our author, rendered one, of the most amusing: we allude to the introduc-

tion of the fever into Cadiz, and in other parts of the province of Andalusia.

It appears, that about the latter end of July, Mr. Flores Proto Medico, of Cadiz, was informed, that a disease of a suspicious nature prevailed in the island of Leon. On the 29th he visited the place, and recognized the malady to be the yellow fever. On the 31st of the same month, the ship Asia arrived from Vera Cruz, after having touched at Havannah. On the 1st of August, the medical junta, composed of Messrs. Flores, Arejula, Ameller, and Coll, visited the island, for the purpose of ascertaining the real state of the city: but, from the report of the physicians, they were led to the belief, that the prevailing fevers were the usual bilious remittents of the season, presenting rather more than usual malignity. On the 18th of August, however, the true yellow fever was discovered in the various hospitals of the Isle of Leon, and more especially in that of San Carlos. From this period, the fever extended itself rapidly. On the 24th, it prevailed in Cadiz—and on the 27th, at Santa Maria and Xeres.

Concerning its origin, our author makes the following observations:

"Whatever reasons the Spanish physicians may have for thinking the yellow fever endemic in Andalusia, they have unanimously considered it, every time it has attacked a great mass of population, as having a foreign origin, and either imported in ships of the royal navy or in merchant vessels. Such was the prevailing opinion in Cadiz, with respect to the epidemic of 1819. But although unanimous on this first point, the physicians were far from being so, as to the vessels or men from whom the fatal present was derived. According to Don Gonzales, a small American vessel had clandestinely landed some goods in the isle of Leon. Those who received them soon died, and the disease of which they perished having broken out ten or twelve days after in a neighbouring house, caused many more deaths. From this point, extending gradually and slowly, it was confined during the first month, to a part of the city called *el barrio del Christo.* About the same time, a vessel from Calcutta, the *San Julian*, entered the bay of Cadiz, and two of her crew having landed in the isle of Leon and

lodged in the same place with the Spanish smugglers, were like them attacked with the disease and died." P. 56.

Next our author produces the following certificate from Dr. Cabanillas, on which, however, he seems to lay very little stress, as well from his not having been told when the doctor had obtained the information therein contained, as from the little probability of truth in the report itself.

"From official information we learn, that the yellow fever was introduced into San Fernando (Leon) by a boat from Tarifa, laden with oranges—under which, the captain had concealed a certain quantity of cotton received from an American vessel. This cotton he landed in el Barrio del Christo, and almost immediately after, himself, together with his sailors, and those living in the same house, died, &c."

Another of the doctor's friends (his name is, however, not mentioned) told him the following extraordinary story, which, notwithstanding its length, we think it indispensable to transcribe, together with the remarks of our author. From these our readers will form, we have no doubt, some idea of his *great* knowledge of the laws of morbid action in general, and of contagion in particular, and of the solid foundation on which he builds his theories. But let us proceed:

"You wish to know from whence we derive the epidemic of this year—read this letter: it is dated from Calcutta—from it you will learn, that during the last two years (1817, 1818) a cruel disease, which *I consider contagious,* ravaged the coasts of Coromandel, extending from Calcutta to Pondicherry, and has caused there great mortality, as mentioned in the English journals, Morning Chronicle, Star, &c. &c. I must add, that this disease is designated in these journals, and by the English in India, under the name of cholera morbus. But as it still prevailed extensively on this coast of India, at the time of the departure of the San Julian, and was seen by the surgeon of this vessel, *I can assure you, on his testimony, that the cholera morbus which reigns perhaps equally in India, is the true disease of Siam, the true original yellow fever.* The San Julian does *not* appear to have had, during the voyage, any sick on board, yet I can assure you that five or six days after her arrival, those in Leon, with whom the mate and two of the sailors of this vessel lodged, died. The mate and sailors soon met with the same fate—and such is the true and sole origin

of the disease which has desolated us. This secret is perfectly known to the medical junta: but reasons you may very easily guess, compel the administration to conceal it. They are apprehensive of being considered imprudent. You will remark, that the San Julian arrived on the 26th of June last, and so early as the 29th of July, Mr. Flores discovered the yellow fever in the Isle of Leon. As to the ship Asia, coming from Havana and Vera Cruz, and which was thought in France to have imported the disease among us, this vessel arrived as late as the 30th of July, consequently subsequent to the first appearance of the disease. But supposing, that the yellow fever had not already existed in the Isle of Leon, and had not reached us from that place, this last vessel would, in all *probability*, have sufficed to intr duce it."—P. 57.

On this marvellous piece of information, derived from we don't know who, our author makes the following very *sagacious* remarks:

"From what has been said, it results that the yellow fever of 1819, *supposing* it to have been imported into Spain, was not introduced by the ship Asia, as is proved by a simple comparison of dates. But was it carried thither by the sloop mentioned by Doctor Cabanillas? or by the San Julian? In other words, was it derived from America, or the East Indies? This is a question difficult to solve. First—Because it is far from being proved that any vessel from America had arrived at Tarifa, having communicated with a sloop from San Fernando. Second—Because, admitting the truth of all this, we cannot conceive how this vessel could have given cotton to the captain of this Spanish sloop, without giving some to smugglers at Tarifa, and without infecting this port. Unless indeed we suppose, against all probability, that in this small quantity of cotton consisted her whole cargo, or that the miasma, with which it was impregnated, was contagious only at San Fernando, and not at Tarifa. It is therefore not *probable* that the disease was derived from this source."

Here we would remark, that we consider the introduction of the fever into Leon, from this American vessel, not only *improbable*, but totally *impossible*—and that our author, without the display of so much reasoning, could, in a very few words, have come to the same conclusion. The fever of America, he tells us, is never contagious—and hence, it is quite manifest to us, cannot be transmitted in cotton, which alone serves as a sufficient refutation of Cabanillas's

official story. We would likewise here call the attention of our readers to a little apparent contradiction. The author says, we cannot think that the miasma contained in the cotton was only contagious at Fernando, and not at Tarifa. Granting this—in other parts of his work, he states that the disease was contagious in one portion of Spain, and not in others. Consequently supposing the existence of the miasma to have been real, and conceding to him the correctness of the opinion he *sometimes* entertains on the subject, the fever might have appeared in Leon and not at Tarifa.

But he proceeds to enquire whether the epidemic was introduced from Calcutta by the San Julian:

"More fortunate than the Asia, this vessel, during the passage, had *not lost any of her crew, nor even had any one of them sick:* and from the intelligence I have lately received in France, I learn that the cholera morbus which reigned epidemically on the coast of Coromandel was not contagious. Notwithstanding some striking features of resemblance, by which it is *rendered analogous to the yellow fever,* the description I have received of the cholera morbus does not permit me to confound it with the former. *Although similar, they constitute two very distinct diseases.* To maintain that the one arises from the other, would be to establish as a positive fact, what the most wise minds would fear to propose as an hypothesis. As every thing in medicine can be explained in several ways, whenever we wish to elucidate an ambiguous question, to prefer without reason one explanation to the exclusion of all others, is an unpardonable temerity. In the present case, for example, it is absolutely *possible* that a disease which is not contagious in one climate, may possess this quality in another, and hence the yellow fever, *which is not contagious* in America, was eminently so in Senegal in 1778. It is *possible* that the men and effects of the San Julian may have *imbibed* in India a germ, which was only to be brought into play (*éclore*) in Europe, under an analogous sky, and in analogous places, but among men differently predisposed. Can this appear more extraordinary than what was observed at Otaheite and St. Kilda? It is *possible* that the least difference in the internal dispositions, may give to any disease whatever, properties which, without, it would not possess. Notwithstanding all, which a number of practical observations have taught us on the subject, what do we know of the transformation of diseases, arising from individual predispositions. It is therefore *possible* that the

yellow fever of 1819 was introduced from the East Indies: but at the same time also *possible* that it originated spontaneously in Andalusia, without germ and previous contagion—for the first time it appeared in this world, it necessarily arose of itself, in a similar way that it arises at Curaçao, Jamaica, &c.: in the way that it twice originated spontaneously, and with a contagious character, about sixty or eighty years ago—Where?—in some parts of Switzerland, and consequently far from the sea, and almost in the centre of a large continent.

"From all this it appears, that after reviewing the whole *series of probabilities*, for want of sufficient proofs to decide, I neither admit or reject any—and in so doing, I imitate my excellent friend Dr. Arejula, who, hesitating in the midst of different hypotheses on the subject of the origin of the yellow fever of 1800, declares that he has not found one with which he is completely satisfied, and that he still remains in doubt. The physicians of Cadiz are nevertheless convinced of the truth of what I have mentioned concerning the ship San Julian, *and I must confess that this opinion appears to me to be the most plausible of all.*"

The doctrine of our author may be thus stated. The cholera morbus, a disease proved beyond doubt to be not contagious, ravages India. The San Julian arrives at the isle of Leon, from Calcutta, in a healthy state, without having had any one sick on board during a passage of four months—the yellow fever appears in that place about the same time of the arrival of this vessel, and *those who land from her, not those who remain on board*, are seized with the disease and die, from all which it appears *most plausible* that the ship introduced it: or, in other words, a disease which in India is cholera morbus, and not contagious, can be converted into yellow fever when transplanted into Spain by healthy men in a healthy ship. Than this, we cannot conceive any thing much more outrageously absurd. In a man holding a certain rank in his profession, and from whom, on account of his preceding labours, we were entitled to expect something, bearing at least the stamp of common sense, a sentiment of this nature is really unpardonable. Any comment of ours to demonstrate the erroneousness of his notions, is wholly unnecessary, since we are persuaded the merest tyro in the medical profession

will laugh at this sudden *transformation* of *cholera morbus* into *yellow fever.* Nor do we feel disposed to treat with particular attention, or to enter into a formal refutation of the extravagant and idle stories he may be pleased to receive as positive facts from any of his *anonymous friends.* We would only remark (and Mr. Pariset may consider this as an *hypothesis well worthy of investigation*), that by making use of this same mode of reasoning, it would not be difficult to prove that the yellow fever of Spain may, if transported into Siberia, be converted into small-pox, in which place the *metamorphosis* will be favoured, not only by a different predisposition in the inhabitants, but likewise by a different sky!

Confined at first to a quarter, called *el Barrio del Christo*, the disease, towards the end of August, began to spread in the city, and the number of patients was soon estimated at two hundred and fifty, and in September at two hundred and sixty—about the end of the same month, they had diminished to less than one hundred, and towards the 10th of November it ceased to exist. In the city of Cadiz, the fever began on the 20th of August, and from that period, spread gradually. During the course of September, however, twelve hundred persons were buried, and on the 8th of October, the number of patients amounted to nine thousand five hundred and twenty-six—on the 18th to twelve thousand four hundred and ninety-four—on the 31st to five thousand five hundred and seventy, and on the 15th of November, one thousand four hundred and forty. From that period it gradually diminished, and finally disappeared in the early part of December. It appears that in a population of seventy-two thousand inhabitants, forty-eight thousand were affected with the disease, of which number only four or five thousand died. In the village of Chiclana, it prevailed from the 16th of August to the 15th of December, and during that period occasioned nine hundred deaths. At Puerto Real, it was less malignant. At Santa Maria, it began on the 20th of August, and ended in December, during which period six hundred and eighty-eight died. At

Xeres de la Frontera, the population of which is estimated at forty-five thousand inhabitants, it also began about the end of August, and did not cease its ravages until the 10th of December: one thousand two hundred and sixty-two were sick, and of this number four hundred and eight died.

"From all that has been said," exclaims our author, "it is evident that in its progress, the yellow fever has observed the march of a highly contagious epidemic." "After its development in the isle of Leon, the disease appeared in such places only, where it was carried by persons who had contracted it in its primary source." He tells us, for example, that the fever was positively introduced into Cadiz from Leon and the *plains of Chiclana*, and in Santa Maria by a woman who escaped from Leon. Xeres, according to him, was infected—first, by a soldier from Leon—secondly, by a woman also from the same place, who communicated the disease to two men living in the same house, though she herself appears to have remained in health; and—thirdly, by an Italian, likewise from Leon, who on his arrival, sickened with the fever and died. The place del Anoyo, the street of Pavia, the hospital and the street de la Gloria, were the four parts of the town in which these unfortunate men perished, and from which the epidemic radiated. "With this circumstance, very worthy of our attention, that the street de la Gloria is *small*, *narrow*, *filled with taverns*, *and inhabited by the poor and by strangers. In this part*, *the disease manifested its greatest violence*."

These, according to our author, are proofs sufficient, that the fever in its progress observed the march of a contagious epidemic, and that all those places we have mentioned, were indebted for the disease to the circumstances enumerated. We are far, however, from entertaining the same opinion. His proofs appear to us not at all conclusive, and we would here remark, that in the topography of the country—in the marshes of the plains of Chiclana, and the filthy nature of the streets of Xeres, he might have found a more rational cause for the origin of the epidemic than in these ridiculous stories, which he has not even thought proper to

cloak with the appearance of official information. But with these *positive* proofs he does not rest satisfied—and to establish his opinion on the strongest foundation, has recourse to those of a *negative* nature, of which we shall here present a few. "*If it be proved* by the preceding facts, that in populous cities, the disease has been violent in proportion to the degree of communication kept up with places primarily infected—it is not less so that in all towns where the intercourse has been less frequent and more restricted, the fever has been confined to those only who had carried it—and in every place where the communications have been intercepted in time, the disease has not occurred. Thus, for example, four men left Cadiz, two for Vejer and two for Conil: as they carried with them the germs of the fever, they sickened with it and died, without, however, communicating it to any one—because they had been wisely separated from the rest of the inhabitants. Again, the village of Scipiona, in the neighbourhood of San Lucar, and the little town of Utrera, situate between Xeres and Alcale, near Seville, preserved the most perfect health, because, from the beginning these two places ceased all intercourse with the surrounding country, or at least submitted all external communications to most rigid formalities.

"In 1800, Chipiona had by the same means preserved itself, whereas, from not having taken the same precautionary measures, Utrera, Vejer and Conil, suffered as much as the other infected cities."

In opposition to all this, we shall remark, that it is next to an impossibility, in Spain as well as in every other country, to intercept so exactly the communication existing between different villages, as to prevent *all* persons coming from the infected district, from mingling, especially when the infection is recent, with the inhabitants of those places which are free from the disease. How is it learned that an epidemic prevails in any situation? Some one must have arrived from the place to announce it, and if so, a disease eminently contagious, as the fever of 1819 is represented to have been, must surely have found its way to all the neigh-

bouring villages, and every precautionary measure afterwards adopted, to prevent its introduction, rendered useless. But even granting these latter to be possessed of all the power imaginable in preventing this introduction, how can we for a moment suppose them adequate to the task of arresting the progress of a fever, depending on a germ so subtile, that even after the death of a patient, the effluvia arising from his body has killed a bird, flying in the air, with as much dexterity as an expert sportsman: and moreover affected the fish at the very bottom of the sea—for so appears to believe our author! As to the case of those men who at Conil and Vejer sickened and died of the fever, without, however, communicating it to any one—can we for a moment entertain the thought, that the mere keeping them aloof from the inhabitants, could have prevented the *subtile contagion*, had it really existed, from affecting *some one?* Were they permitted to die like rats, without medical or any other kind of attendance? This we cannot believe! Did they communicate the disease to the medical attendant, nurse, or any one else? Neither is to be supposed, since had they done so, our author, ever anxious to furnish support to his precious theory, would not have omitted to notice it. This, therefore, will go to prove that the disease was not transmitted in the way mentioned by him, and to show *positively* that the fever was not contagious.

It is said that Chipiona, in 1809, was preserved by intercepting all communication, and that the same measure had previously succeeded in 1800. This preservation, however, we can very readily explain, by adverting to the high and advantageous situation of this village, which, according to Berthe himself, served as much for protection during the epidemic of 1800, as did all the precautions adopted by its inhabitants to seclude themselves from the surrounding infection. Utrera, Vejer, and Conil, in 1800, suffered much, whereas in 1819, they entirely escaped. These circumstances are of course attributed to the measures taken in the latter epidemic, and formerly neglected. But who, having studied the laws of infection, has not learned, that for the

development of an epidemical disease, depending on a local cause, a number of circumstances are requisite, which being all present, will generate the fever, but being absent in whole, or in part only, will prove inadequate to such effect.

A circumstance, which in the mind of our author seems to have made no little impression, but which we conceive, so far from proving the contagious nature of this epidemic, establishes directly the contrary, is, that birds of passage, as well as domestic animals of every species, suffered from the disease in a remarkable degree. The same thing occurred in 1800, and we are informed that in the museum of the College of Cadiz, many stomachs of these animals, especially fowls, are preserved, which were found in a state of complete gangrene. After relating, *on the authority of the captain of a sloop*, that the disease, in the isle of Leon, commenced its ravages on cats, dogs, and pidgeons—that these had died with genuine black vomit—that finally it was communicated to the human species, first attacking children, then grown persons—and transcribing a note, containing some remarkable cases of the sort, from Arejula's treatise on the yellow fever, he exclaims,

"To what singular conclusions on the source of this epidemic will not these facts lead us? If animals are first attacked, and men only affected after them, would we not be *warranted* to infer, that the generative principle of the disease is *purely local*—that it resides in the economy of the animals themselves, or in the quality of the soil on which they live—and that, in both cases, this principle is formed and *developed by the continued heat of the sun*, which determines either in the organization of the animal, or in the vapours escaping from the earth, combinations hitherto unknown? Where, consequently, would be the necessity of recurring to foreign miasmata."

Next, our author proceeds to inquire into the nature of yellow fever. "From the state of the pulse," he says, "and also from the progress and duration of the disease, it is manifest that it appertains to the class of *continued remittents*, and that it is *eminently acute*. On the other hand, the prostration, and sometimes total loss of strength, prove,

that this fever partakes of the nature of typhus, or rather, that it is itself a true typhus." Here, consequently, we have a *true typhus*, accompanied with a *strong, full, hard pulse*, and the other symptoms of an eminently acute disease; and all that appears to demonstrate the existence of this state of true typhus is the *muscular debility*. Can this latter symptom, which accompanies almost all highly inflammatory diseases, the puerperal fever for example—prove them to be of a typhous nature? Has not experience shown the fallacy and even danger of such a notion? But perhaps he is not acquainted with what is understood by the term *indirect debility*. If so, we would advise him, when he publishes a second edition of this volume, to study with attention any of the elementary and popular works on medicine. He will perhaps in them, find, that whenever in the commencement of an acute disease, symptoms of high arterial action are accompanied with those of great muscular weakness, these latter are to be overlooked, and that the only way of removing them and of preventing the disease assuming a typhous character, is by the use of active depletion, to overcome the former.

The next point of which he treats, as characterizing the disease, is its contagious nature. This, according to him, is proved by the fact, that the fever generally commences in one spot—and from the first patient, is communicated to the nurses, friends, &c.; that it next reaches the adjoining rooms—house—then the neighbouring streets, and finally the whole city. He further states, that the march of the disease is influenced by the direction of the winds, as much as by the degree of communication existing between the patients, or those living in their atmosphere, with the inhabitants of other parts of the city—in proof of which he makes mention of the effects of processions in aggravating the disease, and augmenting the number of the sick, as was evident at Cadiz in 1800, and Antequerra in 1804.

"It follows from what has been said, that the cause of the disease, whatever it was, existed neither in the regimen nor in

the condition of the atmosphere, since, with respect to the first, it was similar in all places, or else different in different places. If similar, why did not the disease appear at the same time in different situations? and if different, how can we suppose causes so dissimilar to produce effects so very identical? On the other hand, the atmosphere, possessing every where the same qualities, and producing on the organization similar dispositions, a disease depending on a cause so general, would have appeared at once in different situations, and not have been communicated from one place to another."

These arguments of our author, to prove the disease contagious, being precisely those already employed by Berthe in his history of the epidemic of 1800, and these latter having been completely and satisfactorily refuted by Bancroft—we shall not here attempt to show their futility, or how far they go to substantiate the opposite conclusion. On the above quotation, however, we would remark, that we never knew that the atmosphere in all places possessed the same qualities, but were under the impression, that variously modified in different situations, by a variety of circumstances, it could then become the cause of disease, at the same time that the surrounding country would remain healthy. These views, which we believe we entertain, conjointly with most of the enlightened physicians of the age, we could substantiate, by reference to many parts of the work itself, as will hereafter be shown. As for a general morbid constitution of the atmosphere being the sole cause of yellow fever, we think it almost unnecessary to remark, that no author, or at least few, at the present time, maintain this opinion. Many, it is true, have considered its coincidence necessary, but they all refer the principal agency to exhalations arising from local sources of infection. Now, let us examine if in our author we can discover the presence of any such cause in Cadiz.

After stating, *on his own authority*, that there exists a great difference between a bilious and a yellow fever pestilence, in so much as the former depends on marshy exhalations, and not the latter—he says,

"It must be remembered that the head of the bay of Cadiz, and likewise in the neighbourhood of the *Caraccas* and the isle

of Leon, consists of *low damp* grounds, partly intersected with canals, and covered with *coarse herbs.* The inhabitants have there established salt pans, and the masses of salt, arranged in the form of tents, present the image of a vast camp built in the midst of the waves. It is natural to suppose that the evaporation of these marshes, during the heat of summer, would there produce what it produces elsewhere, in Venice, Rome, Rochefford, and in some parts of Flanders and Holland, namely, fevers of a malignant character, of which the yellow fever has been said to be a modification. But neither does the saturated water of the salt pans, nor that by which these are surrounded, and which is continually renewed, possess the same inconveniencies as waters stagnant and charged with animal and vegetable matter in a state of putrefaction; and consequently, when early and late in December 1819, I traversed these salted morasses, although the temperature continued high, I was not sensible of any smell analogous to that of marshes."

In answer to this last assertion, that salt marshes have not the same influence in the production of fevers as those made up of fresh water, we would appeal to the testimony of those practitioners who, along the vast range of our coasts, have had the amplest opportunities of seeing the most malignant fevers, evidently caused by effluvia, arising from marshes of the above description. Nor are we at all satisfied of the antiseptic powers of sea salt, unless when pure, and in *excess:* Indeed, from some of the experiments of Sir John Pringle, we are led to the conclusion, that when in a small proportion, so far from retarding putrefaction, it becomes a hastener of that process. It is, besides, a fact well known to every one, that vegetable and animal substances putrify much sooner in brackish and sea, than in fresh water, and which is exemplified in some parts of our own country. We have often heard it mentioned by the inhabitants of those districts subject to overflowings of the sea, that when after such accidents, rain falls in any quantity, and is succeeded by a continued and high degree of thermometrical heat, there occurs a more speedy and complete decay of all kinds of vegetable substances, than when the heat is merely preceded by a great fall of rain—and that in the former case, the diseases produced are more general, and assume a character much more malignant. Besides,

do we not know that sea water, when exposed in vessels, casks, &c. putrefies much sooner than rain or river water? Pure salt is undoubtedly an antiseptic, but when in combination with other substances in the sea, it loses that virtue in a very great degree. Notwithstanding, therefore, the high authority of our author to the contrary, for these and other reasons, not necessary to detail, we are led to the firm conviction that the marshes he has described as existing in the vicinity of Cadiz, together with the filthy condition of some of the streets of that city, as mentioned by Doughty and Berthe himself—and the offensive exhalations from the common sewers, and from the port at low water, will seem to account, in the most satisfactory manner, for the local origin of the fever; and especially when we take into consideration the great and sometimes long continued heat experienced during the summer season in the southern provinces of Spain.

It is, however, contended by our author, that neither heat nor the marshes can be assigned as a cause of the epidemic—since, when in 1790, the thermometer rose three degrees higher than in 1800, no yellow fever appeared in Cadiz —and in the second place, because were the assertion correct, disease would appear every year. But these are trivial arguments. It is a fact well known to every one, that to develop fevers, heat must not only be high, but continue so for some time, as was evidently the case in 1800—and consequently, that marshes and filth, unless the thermometer remains at a given degree of temperature for a certain number of days or weeks, will not be adequate to the generation of yellow fever. These are circumstances, which, though unknown to our author, are so well understood by most of the members of the profession, that we consider it a waste of time to dwell upon them with more minuteness.

As a proof of the contagious nature of the epidemic, it is mentioned, that strangers, especially those from the north, are particularly liable to be attacked with it, and he cites many cases of persons who sickened almost immediately on their arrival in Cadiz, particularly when they lodged in

streets where the fever had already appeared. "Now," says he, "*we cannot suppose the organization of these persons to have been prepared for the disease, by the influence of the heat and of local emanations.*" This, we must confess, is rather a singular way of reasoning. We always entertained the opinion, that the disease attacking strangers in preference, especially when they penetrated into the infected districts, went to prove its local origin, and not its contagious nature—and that local emanations, from the action of heat or local filth, *produced* the disease, and did not merely *predispose* to it. As further proof, he relates the cases of many families who left Xeres and Cadiz, and preserved themselves by living in the country. Their servants, however, being obliged *to go to town*, for the purpose of marketing, were infected and died. In some instances families divided—one half of their members remained in town, and the others went into the country: the first were attacked with the disease, whilst the latter preserved their health. "Nevertheless," continues our author, "the *air*, regimen, and *emanations in town*, as well as *in the country*, *are the same.*" He further mentions, that a regiment of dragoons, stationed on low ground, between Leon, Puerto Real and Xeres, and consequently near those places most cruelly ravaged by the epidemic, were saved by remaining in a complete isolation.

"With such an example before us," he exclaims, "how can we not abandon the supposition, that the yellow fever of Andalusia owes its origin to excessive heat and marshy effluvia?"!

He next quotes from Ramon Roméra's History of the epidemic of Jumella, the following facts, which, *in his mind*, go very far to prove the contagiousness of that fever. Roméra informs us, that in its propagation the disease observed a most regular and steady progressive march, and extended gradually until it occupied a circular space or centre, from which it radiated, either by the action of the winds, or by means of imprudent communications—and that at the distance of thirty or forty paces from the infected circle, danger was not to be apprehended. He further states,

that whenever the disease was observed far from this district, it was always found, on inquiry, that the patient had had communication with a person already sick (of course in the infected district) or with one who had remained in the infected atmosphere. Than this, no stronger testimony of the local origin of the fever of Jumella, can be required. The whole disease confined to one part only of the town—all the sick traced to this district—are, in our author's mind, *proofs of active contagion!* Roméra relates, that a young girl, in a healtny part of the town, was infected by her lover, who lived in the diseased spot, and had nursed his dying parents, though he himself was not affected. Even admitting the truth of this report, and further, that this girl could have received the disease from the miasma with which the apparel of the young man was impregnated, the case proves nothing in favour of contagion, since she does not seem to have communicated the fever to her family. But our author, who appears to view it in rather a different light, says,

"I do not know whether I am mistaken: When *facts*, however, of this nature; when such striking *proofs and counterproofs*, present themselves a thousand of times to the minds of observers, must they not be convinced of the existence of contagion? Even had not similar facts been observed in Africa, America, Italy, would it not be sufficient that they have shown themselves in Andalusia and some other parts of Spain, to cause in all the physicians who have observed them, the same sentiment, and to close the lips of their adversaries? Would these latter deny the existence of facts? But to reject the experience of others, authorizes the rejection of our own, and nothing is gained in maintaining, that things should have been seen differently from what they were."

Candour, nevertheless, extorts from him the confession, that the yellow fever has sometimes appeared, even in Spain, without a contagious germ. Thus he informs us, that Admiral Gravina having landed, in the year 1802, five hundred sailors with the yellow fever, they were carried into the hospital and communicated the disease to no one. A number of patients affected with the malady having left Se-

ville, where the fever was *highly* contagious, and gone to Cordova, Carmona Ecija, and Alcala, and though many of them died, no instance of contagion was found. He was told by Dr. Mocino, that in the year 1804 an epidemical fever reigned at Ecija, consisting of intermittents and bilious remittents, and that a number of cases of the genuine yellow fever were observed at the same time to be void of contagiousness. These latter cases, however, our author, on the simple authority of Arejula, denies to have been the yellow fever, and asserts them to have consisted of remittents produced by neighbouring marshes. By Ramon he was informed, that whilst in 1804 the yellow fever was spreading rapidly by *contagion* at Carthagena and Alicant, a number of persons from those places arrived at Murcia with the disease, and that though the city was filthy, they did not communicate it to the inhabitants, with whom they kept up constant intercourse, and that the same thing occurred at Alicant, a village of the neighbourhood. In 1811, however, the first patients who arrived at Murcia with the fever, imparted it, and it spread very extensively. Alicant was ravaged with the *contagion* in 1804, but though in 1811 some patients were admitted there and died with the black vomit, no one was infected by them. From all this it follows, that in Cadiz, Murcia and Alicant, the yellow fever has *sometimes* been *contagious* and *sometimes not*. He therefore admits, with Sprengle, that there exists two species of this fever. The first, as described by Jackson, Hillary and Rush, is more contagious; whilst the second, exhibiting precisely the same train of symptoms, is eminently so, as was observed during the last century in Malaga, Cadiz, Minorca, and lately in Andalusia, Murcia, Valencia, &c.

He further remarks, that at first it was supposed, the former only appeared between the tropics, whilst the second prevailed beyond these limits and in high latitudes.—This assertion, however, according to him, is void of truth, for it was contagious in Senegal in 1778, and has sometimes been known not to possess this quality in Spain, and even as far as the 45° of north latitude. Confessing that the vi-

rus has not been rendered apparent to the senses, he still maintains this to be no evidence against its existence—since, for the same reason, that of small-pox, measles, syphilis, might also be denied. He *believes* that the disease is communicated through the medium of the atmosphere, as well as by contact—that the virus becomes volatilized, spreads in the air, and retains its power of action, even for a great length of time. Thus diffused, it penetrates into the organization, either through the skin or lungs, but most probably is taken into the stomach with the saliva.

That the disease is transmitted through the medium of the air, is proved, even according to him, by the manifest increase in the number of cases caused by processions: here, he says, there is *no contact*, but it demonstrates the great volatility of the contagious germ, and its diffusion in the atmosphere. The energy of the poison diminishes with the number of patients in a given space, and with the *decrease of atmospherical temperature—cold, for example, produces a diminution in its force—in calm weather, on the contrary, it is more active.* He further adduces, in proof of the presence of the germ in the air, its effects on birds and other animals, as was formerly described by Thucydides in the plague of Athens, and by Sorbait in the pestilence of Vienna. He even says, but does not place much reliance on the story, that at Carthagena and Jumilla, *this germ was seen in form of a cloud hanging over the infected parts.*

He thinks that this subtle germ possesses a great affinity for animal and vegetable matter, as well as for woollen clothes, cotton, linen, paper, and finally for alimentary substances, and even supposes that when it attaches itself to these articles, it acquires a double energy.

The following are a few of the many *facts* he has brought forward, in illustration of this assertion. He relates, on the authority of Mr. Flores, that the disease was introduced at Agamont in 1804, by a fisherman, who, having sold his fish to a ship of war from Gibraltar, which place was then infected, received in exchange some cheese and biscuits. He and his family were the first victims of the fever, and from

them the epidemic spread. The idea is entertained by our author, that in this instance the fisherman caught the disease *by eating the cheese and biscuit, impregnated with the poison*, and not from the clothing of the seamen, since his intercourse with them was too slight, and the air too rarified. Mr. Flores told him likewise, that in the year 1813, the ship St. Peter, conveying the viceroy Venegas, arrived at Cadiz from Vera Cruz, in excellent condition, but some of the crew being sent to the nephew of the viceroy, *this gentleman caught the disease from their apparel, though they themselves were not affected*, and from him it spread epidemically.—In 1800, Xeres received the fever from a regiment of soldiers, who, on their march from Cadiz, remained only a night within its walls—the soldiers were all in good health.

He further thinks, that the virus can remain attached for a long time to furniture, walls, &c. and in proof of this, gives the following marvellous examples. In 1801, a regiment was sent from Castilles to Cadiz. This was one year after the great epidemic ; and of 1200 men, of which it was composed, 800 died with the yellow fever, without, however, communicating it to the inhabitants. The poison he conjectures, in this instance, to have remained attached to the walls of the barracks, from the year before. In 1800 there had been only two cases of the yellow fever in Medina Sidonia, both of which came from Cadiz. Having died, their bodies were dragged out of the city and burnt—and the house in which they had resided was closed, and the street barricaded. In 1801, however, this house being re-opened, the disease issued from it, spread in the city, and caused the greatest mortality—and from this circumstance, he is led to the belief, that the winters of 1800 and 1801 were too mild to extinguish the germs of the disease, and that this latter, in many cities of Spain, has been preserved several years.

"The constant appearance, (he says,) of the fever during the warmest months of summer, its gradual diminution, and sometimes sudden disappearance at the approach of winter, have

inspired the Spanish physicians with the opinion that cold, by destroying its germs, acts as a good genius, whilst on the contrary heat acts as an evil one by creating it, or raising it from the torpor into which it was thrown by the winter. But how are we to understand this? What are we to think of this spontaneous generation? If the germ exists, does heat merely predispose more to its action, or else does it augment the energy of the principle? Does it develop it? But to develop it, where can it meet with it, if not in those places in which it had been deposited?"

In the progress of his work, he next inquires, at what degree of temperature the yellow fever can possibly be developed. M. Arejula tells him, (and the same is contained in the work of the latter) that under twenty-three degrees of Reaumer the fever never occurs. He further informs us, that this disease has never been known in Spain, previous to the solstice of summer, in which respect it differs from typhus fever, the small-pox, the plague of Egypt, London, Moscow, Warsaw, Ocksakow—but that, as soon as the sun begins to retrograde, it appears, and ceases its ravages at the solstice of winter. With respect to the winds of Cadiz, he makes the following observations:—

"The easterly winds, during the summer, exercise the most deleterious influence. It is the Chamsyn of the Arabs, the Sirocco of Naples, the Catia of Carracas. This wind burns the ground, and dries up vegetables; gives a remarkable expansion to the blood; irritates the nervous system, and blows on Cadiz both diseases and crimes. It uniformly exasperated the yellow fever, and if it does so, it can surely *tend to produce it.* The truth is, that during the great epidemics of 1800 and 1819, it blew constantly for a period of three months. Its interruptions were but short, and by our friends we were informed, that it constantly kept them in a state of unpleasant drowsiness. The north-western winds are dry and cold in Cadiz, and although they proved favourable in moderating the temperature, they were nevertheless remarked to aggravate the second stage of the disease."

On the subject of the capacity of the system to resist a second attack of the fever, which was particularly observed in Spain, he remarks:—

"Here we can perceive how much things the most similar in appearance, differ in reality. Almost all physicians seem to

consider the yellow fever of America and Andalusia as absolutely identical. It is true that in general, those who had had the fever in America, or who had resided a long time in that country, were exempted from it in Cadiz. So far then the two diseases retain their identity. But independently of the case of father Labat, (who asserted that he had had it repeatedly) more recent examples demonstrate that the same subject can have the fever twice in America. It is moreover proved by facts, that after having had the disease in America, a person can be affected with it in Cadiz—whereas, the reverse has never been known to occur. This circumstance, observed by M. Arejula, has never been contradicted by experience, and unless ultimate observations prove it to be incorrect, we must, I believe, conclude either that the two fevers are different, or else that the organizations are different in Europe and America, without however being able to point out the nature of this difference."

He mentions, that in Spain, the fever never extends farther than thirty-five or forty leagues from the coasts, and does not always follow the course of great rivers. The explanation he gives of the first of these facts, appearing to us rather singular, we will here present it in detail. As the disease, says he, appears only between the solstice of summer and that of winter, it occupies a great portion of the time of its duration, in passing from a sea-port to an inland town, and arrives in this last, at the moment when it must disappear. Thus, in 1804, the yellow fever began at Malaga on the 29th of June, a few days before, or perhaps during the solstice, and only appeared at Cordova, its farthest limits, on the 28th of September. On its *way* to this latter place, it reached Montella on the 11th of August, La Rambla on the 22d, and Espejo on the 27th. He remarks, that in its propagation in the interior, the disease might first attack the city furthest from that primarily infected, provided it were situated more in the *direction of the winds*.

He observes, that during the last twenty years, the yellow fever has prevailed five times in Cadiz: in 1800, 1804, 1810, 1813, and 1819—that the first and last of these epidemics were most fatal and contagious—and that the disease in 1804, 1810, and 1813, was confined to the city.

The fever of 1804, which was brought from Malaga, was mild, and, notwithstanding that the communication with

the surrounding country was kept up, confined to the city. Now it appears to us rather extraordinary, that a disease *sufficiently contagious to be carried from Malaga to Cadiz*, should not be sufficiently so to be communicated out of the walls of the latter place. The epidemic of 1819 was not only more contagious and fatal, but likewise more ataxic, which, he says, depends on a greater predisposition in the inhabitants, arising more from some modifications in the atmosphere, than from regimen—or else it must depend on a greater force in the contagious virus. This last opinion is adopted by the Spanish physicians—they believing that the fever, when it *comes from the East Indies, is more fatal than when it reaches them, after being modified by passing through the American system.* This is the reason, according to them, why in 1819, it proved so malignant—since it came from the East Indies !—The proofs of this origin we have already seen !

It is acknowledged by our author, that sporadic cases of the yellow fever sometimes occur, and are found *not to be contagious.* But he accounts for them by *supposing* the germ to have remained inactive in the system from the year before, and even in some cases, for many years, and that thus pre-existing, the malady has by some cause been excited into action. Extending this very *beautiful theory* a little further, he remarks that should *many* persons retain this germ, the disease breaking out in them after a certain lapse of time, *may become epidemic in Spain.* But the fever, according to our author, is sometimes contagious, sometimes not. This circumstance he attributes to the fact that a very malignant case can produce a mild one, and vice versa—which depends on a difference in the organization of the individuals attacked. Now, this taking place at the same time, in a great number of persons, causes the disease to assume in one year, a more malignant character than in another.

Comparing the early yellow fever with that of the present day, he remarks, that formerly, the fever was sudden in its attacks, and contagious—that it spared Creoles, and those

accustomed to the climate—that the dissolution and rupture of the capillary vessels, deep and superficial, sudden and profuse hemorrhages, both internal and external, were its prominent symptoms; that the yellowness of the skin was only first observed one hundred years ago, by Pouppé Desportes—and finally, that it attacked the same individual more than once. On these characteristics of the disease of former years, our author makes the following remarks. The invasion of the disease is not at present sudden, though it still presents this character in many cases, especially in Andalusia. Secondly, that it now is not susceptible of repetition, which he accounts for, by adverting to the action of the atmosphere on the constitution of those individuals dwelling in warm climates. This, he says, constitutes a great point of difference between the old and new yellow fever—since that of former years attacking several times, (for so said Father Labat,—an old Jesuit, ignorant perhaps of the first rudiments of medical science)—whereas now, the same fever, in Spain, Vera Cruz and some other parts of America, attacks only once ; whilst it continues to possess the power of repetition in the United States, and *sometimes* in the Antilles—and all this has been discovered through his philosophical researches ! Again—the yellow fever in the West Indies continues to spare natives, whereas in the United States and in Spain, it attacks indiscriminately both natives and strangers. But although he attributes this to the very variable nature of the climate and great difference of seasons, he still inclines to view it as an essential difference in the character of the fever.

He, moreover, adduces as a point of distinction in the fever of North America and of the Antilles, the fact, that between the tropics, it does not extend beyond the coasts, whereas in the United States, it has been known to appear far in the interior of the country. But we can no longer stop seriously to examine these notions—our patience is at an end—and as we feel persuaded our readers will unhesitatingly pronounce them to be a tissue of absurd and idle speculation—swe here, and without comment, drop them,

and turn to the consideration of those causes he assigns, as capable of engendering the fever in different parts of the Americas. Between the tropics, he says, the yellow fever is caused, in the first place, by the action of heat on healthy subjects; secondly, by the irritation produced in the system by violent exercise and excesses of all sorts; thirdly, by the debility caused in many subjects, by their extreme poverty; fourthly, by the morbid impression produced on the system, by cold nights and rain; fifthly, by the dampness which accumulates in a still and over-heated atmosphere; and sixthly, by a combination of all these causes. In the United States, on the contrary, the disease is produced *solely* by pernicious effluvia, arising from the sewers of the cities, and from marshy grounds, during the summer heats. Now, from this, and from what we have already mentioned, together with the fact that the yellow fever is not contagious in the United States, as is proved in his mind, by the writings of the late Dr. Miller of New-York, he comes to the conclusion, that the disease, notwithstanding *a manifest sameness in its symptoms, is not identical in all parts of the world.* Here, then, we have three distinct causes for the same disease: in the West Indies, the action of the sun; in the United States, pernicious effluvia; and in Spain, an active contagious principle—all three producing the same train of symptoms, *but different diseases*, which, when transported from one place to another, may undergo a change of properties, at the same time that they retain an identity of phenomena. It appears that Dr. Miller, the only author of this country, with whose writings he seems to be acquainted, has, by the force of his arguments, and the conclusiveness of his proofs, convinced him of the non-contagiousness of the yellow fever of America. But here again, we find a manifest contradiction with himself. What, we would ask, has Dr. Miller proved? He, and a host of others, whom we think it unnecessary here to name, have demonstrated, as far as demonstration could go, that the arguments of their opponents were wholly untenable, and the proofs by them alleged, utterly destitute of force. These

arguments and proofs of our contagionists, are exactly those employed by Dr. Pariset himself, to prove the disease of Spain contagious. Now it strikes us as evident, that if they have been shown here to be incorrect, they cannot be viewed in a different light in Spain, and that in quoting Dr. Miller, and acknowledging our fever non-contagious, our author, contrary to his intentions, has only added another to the many and glaring inconsistencies with which his work abounds.

Next, he proceeds to remark, that as the yellow fever is produced in America by morbid exhalations, and as it is not contagious, it must be considered endemic in that part of the world. But, is it endemic merely because it is not contagious? This he does not believe; for, says he, the oriental plague is both contagious and endemic. Now, although the fever is endemic in South America and the United States, it is so in a different way. In the United States, as in Spain, it is only endemic between the equinoxes, in summer and fall, and does not spare any one, whereas, between the tropics it prevails all the year round, and attacks only strangers. This will, probably, in the mind of our author, constitute another mighty difference between the fever of the United States and of the Antilles.

The fever, he moreover thinks, can be transported from Vera Cruz to Europe in merchandize, though the inhabitants of the former place may be exempt from the disease. This only depends, according to him, on the *air* containing some molecules having no action on Creoles, and which may be carried in their clothes, without, however, affecting them, and in support of this supposition, he refers to the black assizes of Oxford. But we would remark that in this latter case, the fever was typhus, a malady, which, according to himself, bears no resemblance to the yellow fever, and which constituting in fact a very different disease, cannot serve as basis of comparison. But he further supposes that the fever may be carried from the United States to Europe, and even gives some instances of this supposed communication. Now, to us it is a question not a little difficult to solve, how

a disease void of any contagious principle in one country, can be transported from that very country in merchandize or wearing apparel. But we are probably not so deeply versed in the laws of contagion and of epidemics, as would be required to unriddle these difficulties, and as it appears that he is to this moment the only one capable of understanding, and of course of solving them, it is to be hoped, that he will at some future period, favour us with his luminous ideas on the subject.

We have now brought this article to a close. It has been extended far beyond our original intention, and conducted with a minuteness to which, for its merits, the work had no claims. But when a man, whatever may be his reputation, writes a book on a subject he does not comprehend, and thrusts it before the public, and this very book, and this very man have influence sufficient to cause quarantine laws, the most obnoxious to commerce, to be passed in a country in opposition to the opinions of Devize, Valentine, Lefort and others, he deserves thus to be arraigned, and to have his errors exposed, and his arrogance rebuked. Whether we have with any success performed this task, we will leave our readers to determine. We shall, in conclusion, only remark, that the doctrine he has endeavoured to support (we allude to the contagiousness of the yellow fever of Cadiz) has, since the establishment of a free press in Spain, been combated by several writers of great respectability in that country. Of these we shall only mention, Don Francisco Salva, of Barcelona, and Don Alphonso de Maria, professor of medicine and surgery in Cadiz, the latter of whom has written a very excellent work on the epidemics, which in 1800, and 1819, desolated the province of Andalusia. We do not despair therefore, now that a monkish government no longer possesses the power of sending an author to the inquisition, merely because he avows opinions on a subject, different from those it wishes to be promulgated, of seeing in due season, every enlightened view concerning the yellow fever of Spain, rest on a foundation as solid as the subject has been placed in this country through the labours of Rush,

Miller, Caldwell, &c. in despite of the ingenuity, talents, and industry exerted by Hosack and a small band of auxiliaries.

Art. XII. 1. *An Account of the Varioloid Disease, which has lately prevailed at Edinburgh, &c. &c.* By John Thomson, M. D., Professor, &c. &c.

2. *A History of the Variolous Epidemic, which occurred in Norwich, &c. &c.* By John Cross, Member of the Royal College of Surgeons in London, &c. &c.

To redeem a pledge which we made on a former occasion,* we now propose to offer to our readers some account of the varioloid disease, which, of late, has attracted such general attention, both in Europe and this country. It seems to us, that this undertaking cannot be more profitably executed than by exhibiting a closely condensed view of the most interesting parts of the above elaborate works, on the subject—and, accordingly, this is the course we shall in the main pursue—interspersing, indeed, only some slight notices of the disease as it has appeared in the United States.

What were the struggles and difficulties, with which vaccination had to contend in the commencement, and how slow and reluctant were the concessions to its validity and usefulness, are sufficiently known. To receive at once a discovery, so novel in itself, of such high pretensions, and opposed as it was by the whole tenor of analogy and experience, could not reasonably be expected. It was, accordingly, subjected to the severest ordeal its enemies could suggest, and in this scrutiny, objections were removed, prejudice after prejudice worn away, till conviction took the place of doubt and hesitation, and its triumph was signal and complete.

* Vide the conclusion of the Review of Moore's History of Vaccination, in the third Number of this Journal.

During twenty years, this state of unqualified confidence continued unabated in every enlightened section of the world. It is true, that now and then an instance of failure would occur, from causes not at all to impeach its general power of protection, by which some clamour against it was raised, and some temporary popular distrust created. The profession, however, with one or two immaterial exceptions, seemed insensible to such impressions, and remained, with wonderful consistency, stedfast in their faith. Confiding indeed too implicitly, our discernment to real imperfections became impaired, and even a disposition approaching to intolerance arose, which made it reproachful to seem incredulous, much less to express doubts as to the infallibility of the process. But now, under the strong impulse of a sort of panic terror, we are hastening into an opposite extreme, prepared to confess our errors, and renounce, entirely, that practice which had so lately been cherished and defended, with the vehemence and intensity of blind devotedness. Can this be right? or is such a course reconcileable with what is due to our dignity, to the cause we have heretofore so warmly espoused, or to that community which looks up to us to guide them in this momentous concern, by our science and deliberative wisdom. Every obligation properly operative in the case, directs us to pause, to retrace calmly our steps, and dispassionately to survey the whole subject, to contemplate it in its various lights, to contrast its merits with its demerits, and studiously, by every help, to endeavour to arrive at a just decision. To narrow, as much as possible, the scope of inquiry, which without some precise limitations, would prove exceedingly wide and disquisitive, we mean, at present, to discuss chiefly the question, whether, in the existing state of our knowledge, vaccination is still to be trusted?

That it has sometimes proved inefficient, is not denied, even by its warmest advocates. To this point evidence has, by degrees, accumulated, of such force and certainty as not to be resisted. Yet we are persuaded, that the cases cited are only exceptions to a very general rule—and in a candid

estimate, ought not to be considered as real objections to the practice. The sources of these alleged failures we have, in the article which has already been referred to, fully developed, and shown that, for the most part, they are under our control, and, as proceeding from negligence, may, by proper care, be hereafter avoided.

In relation to the subject, such was pretty generally the sentiment, when some few years ago, a disease broke out in Europe, which has led to some new views—and, ultimately, as must be confessed, to a much lower estimate of vaccination. This disease, designated by the term *varioloid*, from its general resemblance to small-pox, seems to have prevailed, for the first time, to any extent, in Edinburgh, in the winter of eighteen hundred and eighteen. It had previously shown itself in several provincial towns of Scotland, and though exciting some curiosity, commanded no serious attention. Contemporaneously, or nearly so, it raged in England, particularly at Norwich, as well as on the continent, in France, at Geneva, in Italy and Germany—crossing the Atlantic, it appeared the next year at Lancaster, in this State—and now, according to report, is desolating the city of Baltimore.

It is described by one of the best of its historians as attacking three different classes of persons: those who had passed through small-pox, those who had been previously vaccinated, and those who never had either of these diseases—in all which cases it exhibited some common characters. It usually commenced in a vesicular form, or in a papular speedily becoming vesicular, and only became pustular in some few instances in its progress. The pustules appeared sometimes with, and sometimes without a *central* depression. The eruption was irregular in size and form, as well as in the place of its first occurrence, seeming generally to occupy only the surface of the skin. Nearly in all instances it came out in *successive crops*, some of which appeared on the body after the eruption was at the height on the face. It appeared in general, even in severe cases, to have arrived at the height on the face by the sixth day of

the eruption, and in the milder, not unfrequently by the fourth or fifth day. The fluid contained in the vesicles and pustules, appeared in a great many instances *lymph*, rather than *pus*, even to a late period of the disease, and generally dried into horny scabs, covering tubercular elevations of the skin, which in several instances, were followed by pits or depressions of that texture. The eruption rarely had any of the smell peculiar to small-pox. It produced very little temporary blindness, and was seldom accompanied by the symptoms of secondary fever. The eruption, however, was sometimes very extensive, resembling small-pox; and in other cases, there were only a few vesicles of the character of chicken pock. It was remarked that in infants, the *varicellous*, and in adults, the *varioloid* appearance usually preponderated.*

This is the substance of the earliest account of the disease, as it occurred at Edinburgh, and is, perhaps, the most faithful, as it was drawn up prior to the adoption of any hypothesis—which, when eagerly maintained, as in the present instance, hardly ever fails to induce, even with the strongest sense of integrity, a distortion of facts, and to throw a sort of complexional hue over statements. By professor Thomson, the writer to whom we allude, a more elaborate treatise has lately been published, in which his peculiar opinions, presently to be noticed, are delivered in a formal and systematic history of the disease. Declining, for obvious reasons, to follow him in detail, we shall attempt to present, in a mere summary, some of the leading and most important of his matter.

In persons who have had neither small-pox nor chicken pock, the eruption is represented by him as preceded by fever, commonly of great violence, though sometimes moderate, continuing for three days. There is, however, in all these respects considerable difference, and as much so in the nature of the eruption, which partakes of almost every

* Vide Thomson's paper in the Edinburgh Medical and Surgical Journal.

intermediate appearance, between ordinary chicken pock and the most malignant confluent small-pox. Of two hundred and five persons, whom he saw in this form of the epidemic, fifty died, making a proportion of one in four—which is a degree of mortality almost unexampled—and, independent of all other evidence, conclusively shows the violent and uncontrollable nature of the complaint. In those who had previously had small-pox, the eruptive fever, in very many, was severe, and in others so mild, as scarcely to be perceptible. The eruption, for the most part, resembled the chicken pock in its several varieties, though, in some instances, it had the appearance either of the discreet or confluent small-pox. Of this form of the disease he saw, or heard, of seventy-one cases, three of which died, giving the proportion of one in twenty-three. It is worthy of remark, that in two of the fatal cases, the attack recurred in a few weeks after small-pox.

The disease in persons previously vaccinated, seems not to have differed materially from that under the immediate preceding circumstances. In describing it, he indeed employs nearly the same language, though on the whole it may, we think, be collected, that such cases were milder. Even when the fever was *ever* so violent, it almost uniformly ceased on the appearance of the eruption—yet now and then it assumed the shape of some of the worst species of small pox, and run a protracted course. It is consolatory to learn that of three hundred and ten individuals affected in this state, only one perished, and whose death can hardly be ascribed to this cause. Of the above number, forty had the disease a second time, after intervals, varying from a few days to several years. There is only one instance mentioned of its returning a third time, and repetitions of attack were distinguished by no peculiarities. It is however very extraordinary, that a large proportion of those who were attacked with the varioloid disease after vaccination, had been in the interval inoculated with small-pox, or exposed to its contagion without being infected.

As before intimated, this epidemic, (for the identity of

the two diseases is generally admitted,) prevailed nearly at the same time in England, and to the widest extent, at Norwich. It has been very fully described by Mr. Cross, a writer of deservedly high credit, who has interwoven with his narrative, many curious facts, and no inconsiderable share of ingenious speculation. The disease in this position, assumed different characters, from the " mildest to the most severe, sometimes with a few pocks which scabbed in six or seven days, without any perceptible indisposion, to an eruption attended with petechiæ, sloughing, or mortification."

To determine the degree of protection from vaccination, during this epidemic, we are supplied with a very circumstantial account of one hundred and twelve families, whom he carefully watched, while the disease was most vehemently raging. They comprised six hundred and three persons, of whom two hundred, who had undergone no kind of protection, had the disease unmitigated, or as it was considered, genuine small-pox. Ninety-one had been vaccinated previous to or during the epidemic, who were constantly in the same room, and often in the same bed with the variolous patients. Two of them took modified small-pox, and one chicken pock, and these were the only instances of indisposition or eruption in the vaccinated in these families, during the continuance of the epidemic. It is concluded from these data, that not more than one in twenty of the vaccinated will be affected in any way by the utmost exposure to small-pox, and that the modified or varioloid disease will not occur in more than one in fifty. The residue of the individuals of these families, amounting to three hundred and twelve, who had had small-pox formerly, or possessed a peculiarity of system enabling them to resist it, seemed entirely to have escaped from every eruptive affection. Besides an exposure to variolous contagion, several hundred of those vaccinated, from the earliest period of the practice until within a few weeks, were subjected to the additional test of variolation during the epidemic, and in no instance was regular small-pox, as far as he could learn,

produced. During the epidemic, however, six cases of genuine small pox did occur, succeeding to vaccination, three of whom recovered and two died, the one having the confluent and the other the petechial form of the disease. Whether in any of these instances vaccination had been imperfectly performed, could not be determined, though such may be suspected. But allowing the contrary, who can doubt the inappreciable value of vaccination, when he is informed that two deaths only happened among those protected by this disease, in a population of ten thousand, exposed, too, for upwards of a year to the most concentrated contagion—while there were five hundred and thirty-six deaths among little more than three thousand individuals, by whom this beneficent provision had been neglected. What, moreover, is very important in making our estimates in this case, it is stated that there were nearly an equal number of recurrences of small-pox under circumstances, in which it is supposed to have been previously had, though in no one instance did it prove fatal.

This epidemic, which is described as small-pox, differed in many particulars from the disease in North Britain. Concomitant with it, however, several eruptive diseases prevailed, among which was one more closely assimilated to the *varioloid* affection. Though occurring in the *variolated*, it was much more frequently met with in the *vaccinated*, and never, if the term be admissible, in the virgin system. The proportion which this particular form bore to the general disease is not mentioned. We have sixteen cases of it detailed, merely as illustrations of the several varieties which it presented. These need not be cited. They were of different degrees of violence, and sometimes menaced danger, though no death actually took place. In most instances the pocks were indented in the centre, which is regarded as a pretty certain criterion of the variolous nature. This point indeed, is stated to have been absolutely demonstrated by the fact, that wherever such indentations existed, the disease was communicable by inoculation, producing genuine small-pox in the unprotected, and in a small proportion

of the vaccinated, an eruption similar to itself in mildness and duration."

The epidemic on the continent of Europe, was marked by no striking peculiarities. It seems, however, to have been of a less malignant character, though it still attacked indiscriminately, persons in the three several conditions so frequently mentioned. At Millau, a town in France, of eight thousand inhabitants, it destroyed two hundred, while there was no instance of the properly vaccinated having the disease in its genuine state. But it is stated that the modified small-pox was frequent, appearing in the shape of an eruption, " about half the size of small-pox, generally less numerous, always shorter in duration, and sometimes small and hard, scarcely suppurating."

In Holland it prevailed to a greater extent, and with similar results. Nearly four hundred died of it in two weeks in Rotterdam. It is reported that " thousands of the vaccinated escaped entirely"—and even among those few who had " a modified variolous eruption, no death took place." Every part of Wirtemburg within this period, was overrun by the same epidemic, and the appearances in the vaccinated are particularly noticed, which, however, not differing from those already described, we shall pass over without further remark. There were indeed very few instances of this kind, and only two deaths among those " registered as having regularly gone through the cow pock."

Concerning the disease, as it has appeared in the United States, our information is not very precise. The accounts, however, furnished us by two of our correspondents at Lancaster, show it under nearly the same character which it exhibits in Europe. It broke out in November, 1818, and was traced to some German emigrants, who disseminated it, in passing through that city into the interior of the country. The disease attacked indiscriminately the variolated, the vaccinated, and unprotected, though not in the same proportions. Of the first description, there were six cases, of whom none died—of the second, forty—two of whom, very young children, died in convulsions—and of

the third, three hundred and fifty, among whom there were four deaths. This slender mortality, with some other facts, lead us to suspect that the disease was varicella.

From Baltimore our intelligence is still more defective. It is stated, however, to be the common impression, that the disease was imported from Liverpool, though this is doubted—and we learn it occasionally attacks both the vaccinated and variolated, we presume in a mitigated shape, since no death has occurred under such circumstances. The unprotected have suffered much, many being affected, attended with a mortality of about one in six or seven cases. This is the substance of various communications which we have received, separated from a great mass of vague and contradictory statements.*

Connected with this inquiry, the facts are now perhaps sufficiently laid open, to enable us to enter on those speculations which immediately grow out of the subject. The question that primarily arises, relates to the precise nature of the epidemic, whether it really be small-pox or some other affection not obedient to the same laws.

Two hypotheses have been advanced, each of which is maintained with much ingenuity and force of argument. The predominant opinion in Great Britain seems to be, that it is genuine small-pox, which, operating on a system that has undergone the vaccine or variolous impression, produces a comparatively mild affection, to which the title *modified small-pox*, or *varioloid disease*, is applied.

In support of this view, it is alleged that the most lenient and worst forms of the epidemic reciprocally produce each other, either by inoculation or in the natural way, and this is held forth as the *experimentum crucis*, from which there is no escape. But it seems to us to be rather *a petitio principii*, in which the variolous character of the case is assumed and not proved. Whatever may have been the disease elsewhere, it is hardly possible to conceive, that as

* It is to be hoped that some competent person, who has seen much of the disease, will draw up a faithful account of it Of the Lancaster disease, we are in daily expectation of receiving such an account.

it existed at Edinburgh, it could be small-pox. There are several discrepancies utterly irreconcilable with such a supposition, on any ground of correct diagnosis, or with our pre-existing notions. Not to dwell on minor points of difference, it seized indiscriminately on the previously *vaccinated*, the *variolated* and the *unprotected*, by either of these processes, and in some instances there were two and three reiterations of attack in the same person within a very short time. Nor had it the symptoms or character of small-pox in some other respects. Expressly are we told by its historian, that the eruption, for the most part, came out in successive crops—that it was vesicular more than pustular, containing lymph only—that it dried in horny scabs—that in no stage did it emit the peculiar variolous odour—that there was no secondary fever, and that it resembled varicella, which he indeed at first imagined it to be.

Could lesser difficulties be overcome, we really cannot perceive, how we are to account, (allowing the disease to be the small-pox,) for the failure of variolation as a preventive, in such direct contradiction to all former experience. Towards an explanation, it is suggested, we are aware that, at all times, such occurrences were more common than suspected, and that the eruptions hitherto called horn pock, chrystalline pock, water pock, were really the variolous disease in its several modifications. By Thomson, indeed, it is insisted, that varicella itself is only small-pox modified by some original peculiarities of system, or which has become so by previously having undergone either that, or the vaccine disease. It was the general doctrine of the schools, till the time of Heberden, that these two affections were identical, so far at least, as dependent on similarity of source. But on his authority, they were separated, as specifically different, and the distinction has ever since been retained.

Notwithstanding the strong facts adduced, in maintenance of this hypothesis, such as the contemporaneous existence of the two diseases, and their reciprocal production, occasionally by contagion, we are not disposed to acqui-

esce in it. It seems to us, were it true, that such an effect should be constant, and invariable under all circumstances, which does not happen! This is so little the case, that it is notorious in our own city, that within the last ten or fifteen years, varicella has repeatedly prevailed, though small-pox was extinct. The same independent existence of the disease has been remarked elsewhere, and particularly during the prevalence of the late epidemic in Great Britain. As diseases too, are always mildest from inoculation, it follows, that varicella would be more likely to be induced in this way, than casually. But so far from this being true, there is no case on record to this purport, or conversely. Experiments made and repeated with great care, by different persons, show, that chicken pock cannot be excited by variolation, or small-pox by the varicellous virus.

Taking all the circumstances into view, we are much more inclined to the adoption of the hypothesis, which alleges that these recent epidemics have not been genuine small-pox. To this conclusion we are conducted, by the the consideration of the utter impossibility of reconciling the multiplied failures which have taken place, as well in variolation, as vaccination, with all preceding experience of the infallibility, or nearly so, of these two processes. What becomes, on any other supposition than the one proposed, of the evidence deduced from the infinity of experiments which were made in the early stage of vaccination, for the purpose of determining its efficacy. Thousands of individuals, we are told, were exposed after having gone through this operation to the most concentrated contagion of small-pox in hospitals and elsewere, with entire impunity. What too becomes of the test by inoculation, in which formerly such implicit confidence was reposed? No point was seemingly better established, than that an individual having had vaccination, was rendered forever unsusceptible to the action of the small-pox virus. It was the practice, for a series of years, to subject all cases to variolation as criterion, and the result of this extensive experience, was as we have stated. But now the fact is otherwise.

Exactly the same may be demanded with regard to inoculation. The lengthened and accumulated evidence which sustained its efficacy is gone, and we are released from a creed, which, slowly and cautiously adopted, has been cherished with idle credulity for more than a century. Difficult as it may be to suppose, that we have been labouring all this time under so gross a deception, it unavoidably follows, should the disease be proved to be small-pox. But presuming the contrary, or that some entirely new, or modified shape of an old eruption with the general variolous aspect, though in some particulars of a different character, has appeared, we are supplied with an infinitely more satisfactory solution of this intricate problem.

Nor do we perceive, why we should refuse our assent to such an hypothesis. Change is as incident to disease as any thing else, and we learn from our medical records that while some are gradually modified, or wholly extinguished, others are suddenly brought into existence. The venereal distemper is an exceedingly pertinent example. Developed by some fortuitous combination of causes, it has, in the progress of time, undergone a most remarkable revolution in its character and treatment. What is there more extraordinary in the varioloid than the syphiloid affections? That inoculation should no longer afford security against the contagion of variola, is surely not more surprising than the inefficacy of mercury in several of the modern forms of syphilis.

It has been held by the writers generally on the continent of Europe, that the epidemic in question was really a malignant and highly aggravated state of some form of varicella, and on several accounts this opinion deserves attention. Could it be established, it would at once remove all our perplexities—and certain it is, that it is not unsustained by facts. By all it is admitted, though generally there is no difficulty in distinguishing the two diseases, that occasionally varicella assumes a character so imitative of small-pox, as readily to be confounded with it. That disease has more than once appeared in this city in such a guise—and we are informed by Willan, who, of all men, was the most

conversant with the diagnosis of this order of affections, that in six years, he saw seventy-five cases of chicken pock, which had been mistaken for small-pox. The resemblance, indeed, is often so close, that, as we have before mentioned, till within the last half century, they were considered as the same disease. It does not comport with our limits to examine this hypothesis further, and we shall merely observe, that it derives no slender support from the circumstance, that neither variolation nor vaccination affords any security to the attacks of varicella, in which particular it corresponds precisely with the late epidemic.

In whatever light the subject may be contemplated, we can discern no just grounds for withdrawing our confidence in vaccination, and still less for reverting to the process of variolation. We have seen, that in the degree of protective power, under recent circumstances, there is no essential difference—while, in every other respect, the superiority is indisputably with vaccination. Exclusively of various considerations by which it is recommended, too obvious to be noticed here, it is a process mild in its general character, rarely inducing unpleasant consequences, and never proves fatal. But of inoculated small-pox one in two hundred and fifty dies, according to Willan, and several distinguished English writers have made it as one in a hundred.* Distinct from such prodigious mortality, it may be objected to variolation, that it very often entails the most lamentable effects, developing scrofula and other loathsome diseases, causes the loss of sight, and is utterly destructive, by its disfiguration, of personal comeliness.

By variolation, the sources of contagion are, moreover, multiplied, each case proving a new point from which the disease may emanate, so that, though individuals were benefited by the mitigating influence of the process, the aggregate of mischief was actually increased. Calculations made by Heberden, without any reference to this question, show,

* One in six dies, according to the best computation, of natural small-pox.

that subsequently to the introduction of inoculation, *ninety-five* persons died of small-pox in London out of every thousand—whereas the average number antecedently to it, was only *seventy* in the thousand.

What sort of an evil small-pox was, even in this state of supposed amelioration, may be determined, when it is told, that it destroyed nearly fifty thousand individuals annually, in the kingdom of Great Britain only, occasioning every fourth death on the bills of mortality! By vaccination, even limited as it is by vulgar prejudices, this frightful expenditure of human life has every where been greatly abridged; and in some countries, as Denmark, where it is rendered highly penal to variolate, it no longer exists at all—small-pox having become extinct. An extension of the plan by which it has thus been eradicated in one country, will as certainly do it, in relation to the whole world; and by a wise and cordial co-operation in this great work of benevolence, this terrible scourge of humanity might, in a few years, be so completely annihilated, as to leave only behind its name, and the story of its former ravages.

In concluding the subject, we have to state that there is no evidence within our knowledge, to warrant the popular notion, that vaccination is the parent of certain foul eruptions, or that in the progress of time, the susceptibility to variola may be revived in the system. Experience, so far as it extends, confutes, we have reason to believe, each of these impressions, and that, like the rest of the objections to vaccination, they are gratuitous and unfounded. The attention of Willan to the first of these points, and his ample opportunity of deciding it, are well known. By him we are expressly informed, that he is not sensible that any new affections of this kind, have been generated since the prevalence of vaccination, or that the old disorders had become more numerous or virulent. But directly the reverse as to the latter at least, the distemperatures of the skin having diminished. It is also a remarkable fact which he cites to the same purport, that in Gloucestershire, where the vaccine has existed for

half a century or upwards, in which time several hundred had the disease, no such complaints are heard of, or alleged in the case.

As to the second point, inquiries carried on in Britain very extensively, prove that time exercises no influence whatever over the process, the instances of failure not being more in the old, than recent vaccinations.

On the whole, as the result of the best examination which we are capable of giving to the subject, we cannot help recommending an entire confidence in vaccination as adequate to all the purposes which were originally professed. "Time, as we said on a former occasion, has rather accumulated and confirmed, than impaired the evidence of its validity, and such, we are authorised to state, is the opinion of Jenner himself, supported by the official reports of the board of the national vaccine establishment of London. Let it, indeed, be admitted that vaccination is an imposition—all experimental inquiry is futile, our senses are illusive, medical testimony a fallacy, and we must surrender ourselves up to incredulity and scepticism, vacant and unprofitable. But such is not the case, and our prospects of the future are most encouraging. Duly attended to on our parts, we apprehend that all difficulties may be overcome, all cavils silenced, and all prejudices removed on the subject. By adverting to the loose and indiscriminate manner in which vaccination has been practiced, by the heads of families, by the clergy, by old women, and other benevolent, though very incompetent personages, we ought rather to be surprised that the number of failures is so small, in a process which, from its delicacy, requires to be superintended by all the powers of skill and discrimination.

Every good is interwoven with some portion of evil. The singular mildness of vaccination, by taking it out of professional hands, has undoubtedly very much detracted from its utility, and exposed it to some very unjust imputations. But, a reformation may be instituted in this respect. Considering how much our pride is interested in a

discovery, which so gloriously illustrates the character of the profession, it is among our first and highest duties to vindicate it against the caprices of fashion, and fluctuations of opinion, till it is firmly established, to be transmitted to posterity, unimpaired, as the noblest monument ever erected by science for the purposes of human benefit and happiness.

BIOGRAPHY.

Art. XIII.—*Biographical Sketch of* William Baynham, *Esq. Surgeon, late of Essex County, Virginia.*

In no part of the civilized world does the assertion of Cicero, that *vita enim mortuorum in memoria vivorum est posita*, in a general sense, prove less true than in Virginia. That veneration for what is great and excellent in the human character, which elsewhere embalms for posterity the names and actions of illustrious individuals, in this country, appears to be commanded only by the living. Scarcely are the obsequies of the dead performed, and the ordinary exactions of custom complied with by a few indispensable observances, before they are forgotten, and the grave, which closes on the last remains of mortality, at the same time, consigns to irremediable oblivion the recollection and the fame of their most splendid and most noble achievements. It might have been expected, that, in a country to which has been awarded, and by no partial hand too,* the palm over our northern and eastern brethren, in genius and feeling and fancy, and in all those qualities which are calculated to produce eminence in arts or in arms—to bring forth eloquence in the senate and heroism in the field, the influence of the imagination would sometimes have lured us back to the days of our ancestors, and that of the heart led us to perpetuate the recollection of their glorious deeds, by some more durable and more worthy records than those in which they are used to be found. But, if the honours paid to the dead are to be made the criterion of those qualities which have been so liberally ascribed to us, we fear

* See Walsh's American Review.

that we shall be forced to confess, that our genius is ungrateful, our fancies cold and feeble, and the enthusiasm of our praise only to be excited by those who can enjoy, and who can reward it. Excepting the annual tribute doled out to the departed great, with undeviating fidelity, on the 4th of July, when the little that is known of them is repeated with a scrupulous love of truth, and fear of novelty—and the vague and scanty anecdotes respecting them that float in uncertain tradition, and which change their forms as frequently as the clouds that float in the atmosphere, there is no record whatever, where the young Virginian can learn any thing more of the lives and characters of his ancestors, than what is immediately connected with a short period of our history. There is no lasting monument to perpetuate their names, there is no glowing page where he can contemplate and admire their virtues, or where he can learn to imitate their actions. Not a tomb or a stone marks the spot where the ashes of most of them repose, and the pilgrim, whose rare gratitude leads him to seek their neglected graves, esteems himself fortunate if some old grey-headed domestic can point it out among the ruins and the weeds of the ancient and unrevered cemetery. But this is not our only ingratitude. The muse of biography sleeps over their graves, and no pious son has attempted to rouse her from her slumbers, to sing the exploits of his fathers. In short, no where, if we except the life of Washington, by chief justice Marshall, can be found an authentic and regular biography of any one of the revolutionary worthies, who have done so much honour to our state and to the world.

It is deeply and seriously to be regretted, that in a country which affords such excellent and such abundant materials for the pen and the panegyric of the biographer, so few have been found to court this elegant and useful walk of literature. In every other country, and in every age, there appears to have existed in mankind a strong desire to celebrate and to honour the names and the memories of those who have eminently signalized themselves by extraordinary

virtues or by splendid or important achievements. Accordingly, we discover, in the earliest stages of literature, the rudiments of biography in the songs of the bards and minstrels, who sought to celebrate and to preserve in their days, the remembrance of their heroes and the admiration of their feats. Nor does this laudable desire appear to have been extinguished among the improvements and refinements of modern literature. The national biographies of Europe, are swelled with names which were scarcely known during the lives of the individuals to whom they belonged, and the statesman who wields the destiny of empires, and the general who fights battles that a Cæsar might envy, is scarcely more certain of having a biographer, than the skilful mechanic and the unskilful poet—the mountebank, who displays uncommon dexterity in feats of legerdemain, and the empiric who pretends to have discovered a panacea in a new nostrum. We certainly do not wish to see the Europeans imitated in this vicious excess, to which they carry every species of literature. The great only should be honoured—but we honour none. Even in our sister states a praise-worthy disposition appears to exist to discharge the debt of gratitude which we owe to our progenitors. In Virginia alone it is, that the past appears to have no existence for us, and that, occupied exclusively with the present, every man may truly say, *omnis in hoc sum.* Whence does this unworthy apathy arise? Is it thought that virtue has more merit, when it is the result of reflection, than when it proceeds from imitation—and that it is unnecessary to hold out models of excellence to those whose duty, and, peradventure, whose ability it is, to follow the advice of the poet, and *sua sponte recte facere, quam alieno metu?* This view of the subject leaves out the question of the advantage to be derived from biography in the study of human nature, and in the knowledge of that combination of means by which eminent men have been enabled to arrive at the height which they have attained. But we sincerely deprecate that presumptuous and overweening confidence which

leads us to believe that we are invincibly virtuous, nor do we at all envy the insensibility which imagines that it neither can be elevated to great and noble deeds, nor restrained from low and grovelling actions, by the examples of the great and the wise: and even supposing such instances of wilful intractability, or of all-sufficient and unconquerable virtue to exist in particular individuals, it is the duty of those whose province it is to form and reform mankind—our authors and legislators, to consider the world as it actually is, with all its moral and physical imperfections, and to make use of every engine which they can possibly wield for the promotion of virtue, and the discouragement of vice.

Perhaps the cause is to be traced to the proverbial ingratitude of republics, and Virginia is more ungrateful than other countries, only because she is more republican. But this is a solution which is so repugnant to all our love and admiration of republicanism, that even if it were true, it would never be admitted. In our opinion the cause is not very obscure, and the whole of our literary indifference, as well as many other defects in our national character, may be accounted for in one word—namely, politics. It is our exclusive and infatuated devotion to politics which prevents our success in every other branch of science, and in literature, and the arts,—and indeed, which makes us totally neglect them. In Virginia, we are all politicians, and what is more, we are all political writers—from the school-boy who is scarcely out of syntax and the "English Reader," (if haply he was ever in them) to the superannuated and pragmatic dotard, who, with the aid of spectacles, can scarcely read, and who cannot understand his own writings. We are far from disapproving of political studies, and readily admit, that in a republic, where a knowledge of our rights and liberties is a necessary antidote to the insensible encroachments of power, they ought to constitute the fundamental, or at least, the primary branch of education. But we ask, are the honours which attend the noisy and turbulent career of the demagogue the only honours worthy of exciting

emulation, and the capitol the only temple in which a seat is desirable for the ambition of youth? Who will say that the fame of a Davy, a La Place, or a Stewart, is not as enviable as the diplomatic honours of a Talleyrand, a Castlereagh, or even a Chatham? To merit, indeed, even political distinction, in any extraordinary degree, literature and the arts must be united with politics, which, without their ennobling influence, degenerates into a narrow and contemptible art, unworthy of the name of science, in which low cunning and selfish ignorance, usurping the place of knowledge and integrity, by a rapid and inevitable progression, degrade the most high-minded nations to their own despicable level. Whatever is noble and elevated in individuals,—whatever is exalted and magnanimous in nations, and whatever has been worthy of admiration in both, has been the result of that union of all the sciences, and that cultivation of all the faculties, which alone can lead to super-eminent excellence—and it is utterly in vain, for either, ever to wish for or expect permanent political distinction, whilst they neglect the broad and imperishable basis of general knowledge, which alone can render it durable—or despise the elegancies of literature, which alone can dignify and adorn it. It is devoutly to be hoped that this disgraceful apathy will not always endure, and that, unless our political mania is entirely incurable, the rapid progress of literary improvement, so visible in every part of the civilized world, may extend its beneficial effects to us, and give a new and active impulse to that dormant spirit, which now only awakes to brawl amidst electioneering riots, caucuses, and clubs, and that the best talents of the country will not always be permitted to evaporate in toasts and orations, newspaper essays and political circulars.

Perhaps some Plutarch may yet arise, who "fired with a thirst for noble fame, and smit with the love of honourable deeds," shall embody in his faithful and eloquent page, the virtues and the actions of our illustrious ancestors, and thus hand them down for the imitation, the veneration and the

gratitude of the rising and future generations, to the most distant posterity. Nor should his grateful and honorable task be too long deferred. The busy hand of time is daily removing some of those documents from which any authentic biography must be compiled, and covering with a veil of impenetrable obscurity, those events which lose their lustre as well as their reality by the incertitude in which they were involved. Added to this irreparable waste of time, that ignorance and indifference which have, within our own knowledge, already destroyed the most valuable materials, is every day renewing the sacrilege, and with vandalic impiety, sacrificing in the letters and other writings in their possession the record of facts, the maxims of experience, and the lessons of wisdom. This ruthless extermination is proceeding with gradual, but inevitable steps to remove every vestige of the past, and if it is not soon arrested by some enterprising and laborious biographer, will shortly leave him no other oracle to consult than the Babel-tongue of tradition, which confounds all actions, events and ages in chaotic and inextricable confusion,—and will drive him to the unfortunate alternative of either misrepresenting the great, or of permitting them to remain in an oblivion inglorious only to their survivors. Let us not then delay the fabric until the materials of which it is to be built shall have crumbled into dust, but hasten, while it is not yet too late, to redeem the errors of the past by the atonement of a rigorous and unremitted servitude for the future.

Entire as has been our devotion to politics, there still have been found among us a few for whom science had sufficient allurements to seduce them from its thorny paths, and delirious pleasures, to pursue her own sober and captivating graces—a few who, visiting other countries, have acquired an *exotic* taste for letters, and the arts, scarcely known to the true home-bred Virginian—a taste that they have cultivated with a success which, it is to be hoped, will bring new votaries to their shrine. Among the few who have pursued this honourable, and, in this country, singular

course, is the individual who is the subject of this memoir, an individual who, but for his secluded life, and the peculiar taste and habits of his country, which refuse all distinction to scientific skill, would have ranked amongst the most celebrated, as he undoubtedly was among the most eminent men that Virginia has produced.

William Baynham was the son of Dr. John Baynham, of Caroline, and was born the 7th of December, 1749. After having devoted five years of his early life in acquiring the elements of his profession under the auspices of Dr. Walker, at that time considered as one of the most eminent surgeons in America, and thus by a regular and laborious apprenticeship, laid that foundation for future eminence, which unfortunately, by too many, is deemed unnecessary—he was sent by his parents or guardians to complete his education in London, where he entered a student at St. Thomas's Hospital. Here he very early distinguished himself by his assiduity and success in his studies, and acquired so much reputation, that immediately after he completed his course, he was taken into partnership by a Mr. Slater, an eminent surgeon of Margate. Whilst he was in this situation he received an invitation from Mr. Else, the then distinguished Professor of Anatomy at St. Thomas's, to return to London and become his assistant demonstrator. A proposition so flattering, and apparently so advantageous, was immediately acceded to by Mr. Baynham, by the consent of Mr. Slater, who, however, to induce him to continue, offered him an equal share in the partnership. Having returned to St. Thomas's, he still further improved his information, and augmented his fame, acquiring that minute knowledge of anatomy for which he was ever afterwards so celebrated. He succeeded in injecting the *rete mucosum*, and obtained great credit by several anatomical preparations, which are still exhibited at St. Thomas's. His skill and his reputation had both so much increased during his residence at the hospital, that upon the death of Mr. Else, he was run for the anatomical chair, which, though he lost by one vote, he would have obtained by one, but for the absence of two of

his friends, who meant to have supported him. When we consider the prejudices that at all times have existed in England against Americans, and which at that period were increased and fomented by a sanguinary war between the two countries, nothing can show, in a more flattering light, the estimation in which he was held than this distinguished compliment to his talents. An equally high compliment was paid him by Mr. Else, who in his will, offered him the refusal of his anatomical theatre, at 600*l.*—If Mr. Baynham refused it, another favourite pupil and assistant was to have it at 800*l.* and he refusing it, the present celebrated Mr. Cline was to have it at 1000*l.* The two former declining the offer, Mr. Cline, the present lecturer at St. Thomas's, became the purchaser—but Mr. Else, by the manner in which he had graduated the legatees, clearly showed his opinion of the superior merit of Mr. Baynham.

After the death of his friend, Mr. Baynham commenced as an independent practitioner in London, and, at the same time, attended, during the winter season, as assistant demonstrator, in the University of Cambridge, whither his celebrity had procured him an invitation.

Having resided sixteen years in England, he returned to his native country, and shortly after, settled in Essex, wher he continued until his death.—It is not to be wondered at, that having laid such a solid foundation, and brought with him so distinguished a reputation, he should immediately obtain an extensive practice, and support and even augment his celebrity. To enumerate the different operations upon which his reputation rested, would be superfluous. Many of them are already well known to the public, and especially to medical men. There is scarcely any difficult operation in surgery which he did not perform, and with almost invariable success. He particularly signalized himself by several operations for stone, cataract, and extra-uterine conception. A detailed account of a case of the latter, may be seen by reference to the New-York Medical and Surgical Journal. It is no small proof of the superior merit of Mr. Baynham, that, notwithstanding the ob-

scure and confined theatre of his practice, his talents, hidden as they were, in some measure, from the public eye, by the seclusion of a country life, and restricted by a limited population, he acquired a reputation quite as splendid, and almost as extensive, as any of those eminent surgeons who possessed, for the display of their abilities, the advantage of the most populous cities, and the most conspicuous official stations. He was frequently sent for to our large cities, and sometimes even into other states. He was applied to for advice by persons living in remote parts of the Union, and had patients attending him, at his own residence, who were brought thither by his fame from those parts, to obtain the benefit of his skill. We are the more particular in bringing forward these testimonies of his celebrity and of his merit, as it has been understood that some ignorant persons, still more malicious than ignorant, have lately endeavoured to detract from his fair fame, by depreciating his skill as a surgeon and a physician—and, have even gone so far as to assert that he was "a mere empiric."—A charge so notoriously false, would scarcely be worth refuting, were it not for the purpose of holding up to public detestation and contempt, that low and narrow-minded selfishness, that base and envious malignity, which, goaded on by its stings to the destruction of its victims, frequently forgets the impotency of the means employed—and, for its own mistaken gratification, has the injudicious hardihood to fly into the face of the best established facts, and the most notorious truths.

As a surgeon it is *probable* that Mr. Baynham had no superior—as an anatomist it is certain that he had none. The most ample testimonies exist to prove, the respect in which his talents have always been held, both in Europe and in America. He is always mentioned by the several professors of anatomy in our Universities, in their lectures, with the greatest respect, and we have heard a distinguished teacher assert, that he considered him, in the art of making anatomical preparations, only second to Ruysh. But the best evidence of his merit is the concurrent and unani-

mous approbation of those among whom he always lived, and of those, every where, who were *capable* of appreciating his talents. It has been falsely supposed by some, who were probably misled by his great surgical reputation, that he was not eminent as a physician. But if success can be assumed as a criterion of excellence, he deserved the highest credit in the latter character—and accordingly it has been awarded him by the suffrages of those who had the best opportunity of judging of his merit—his own patients.

Mr. Baynham died on Thursday, the 8th of December, 1814, having the day before completed the 66th year of a life, which had been actively and usefully spent in the pursuit of a laborious profession, and in the service of his fellow creatures.—To the superficial observer, who measures the altitude of the mind by the vibrations of the tongue, and considers fluency and eloquence of discourse as the only unerring criterion of talents, he could not have appeared worthy of his high reputation. "There are," says Hume, "among a great variety of human geniuses, some, who, though they see their objects clearly and distinctly in general, yet, when they come to unfold their ideas by discourse or writing, lose that luminous conception which they had before attained." Of this description was Mr. Baynham. He was slow, and not very distinct in the enunciation of his ideas. Entirely unambitious of ornament—talking only to be understood, and never for effect, despising the prismatic glare of factitious refinement, and exaggeratory declamation, by which the feebleness of the idea is so often concealed beneath the splendour of the dress, it is not to be wondered at, that he seldom made a favourable impression on strangers—and that he generally, on first acquaintance, disappointed those whose expectations had been raised, and whose opinions of him had been formed from the report of fame. But though he was not what is called an *esprit fort*, he was unquestionably a man of excellent judgment in his profession, and though not in the words of Bacon "a ready man,"—he was a "full and accurate one."

We have spoken of Mr. Baynham as a surgeon and physician—we now approach that part of his character where envy and malice can no longer wish to seek for unworthy triumphs—where professional jealousy itself will now be willing to make sacrifices of its hatred on the altar of truth, and to pay that tribute to departed worth, that justice demands, and which it can no longer be the interest or desire of any to withhold—that part of his character which, to himself and to his posterity, is of infinitely more importance than all the fame, and all the favours which the world can bestow—his moral worth—his merit as a man and as a member of the great human family. We approach it too with satisfaction, confident, as we are, that it will afford his friends and admirers an opportunity of contemplating a union of qualities upon which their recollections may repose with pleasure. In most of the relations in which Mr Baynham was connected with society, he discharged all his duties in an unusually exemplary manner. He had formed very rigid notions with respect to almost every social obligation, and he was scrupulously exact in adhering to them. That he had some excentricities of temper, must be confessed, but that they have been exaggerated by those malicious observers, who, in the language of Franklin, prefer looking at a man's ugly leg to his handsome one, is also true. His excentricities, like those thin clouds that sometimes darken the face of the day, and obscure its light for a while, were fleeting and temporary—occasional and irregular, but soon dissipated by the genial sunshine of a cordial heart. With an exterior somewhat gloomy and austere, he possessed the kindest and most philanthropic feelings—indeed, few men had naturally warmer hearts—few had more of the real bullion of genuine benevolence, for which the dross of ostentatious charity is so often accepted by the world. In him, the poor ever found a friend and benefactor, ready by the humane offices of his skill to assuage the pangs of bodily disease, or by his beneficent liberality, which sometimes bordered even on munificence, to relieve them from the equally cruel pangs of poverty. Death, which removes

its victims from the prejudices and the persecutions of the world, generally secures for them the impartial rewards of their merit, and posterity, which sits in judgment on their characters, like the Cretan judge, dispenses that justice which they cannot always obtain from their contemporaries. *His* character is now at its bar, with all the evidences of its virtues and defects still fresh in the memory—and if it possess that justice which we have ascribed to it, it must be said that, after deducting the necessary imperfection which is the inevitable condition of human existence, there remained much more than the average degree of excellence which usually falls to the lot of man—while it displayed fewer of those shades which gratify the spleen of the cynic, and feed the malignity of the misanthrope, than of those lights which gladden the bosom of the philanthropist.

P. S. We have received, since writing this sketch, a letter from a gentleman of high medical reputation, both in Europe and America, to whom we had applied for information on the subject—an extract from which we herewith subjoin. Coming from an individual who studied at the same hospital, and who has eminently distinguished himself both as a practitioner and a professor, it must be considered as a most invaluable testimony to the merit of Mr. Baynham.—The liberality too, so inseparable from real talents, with which he has come forward to do justice to the memory of departed worth, furnishes a strong and gratifying contrast, to the calumnies with which envious mediocrity has endeavoured to assail it—and at once refutes and puts to shame its malignant authors.

"He was one of those very few men," (says the gentleman) "whose loss will be a public misfortune, for I know not who is to succeed him in Virginia as a surgeon. In his profession, he was second to Physick only, and nothing but the most narrow, inveterate, and malignant jealousy could asperse his medical reputation. It it true, he never graduated. This arose from his having studied in London, where there is no university capable of conferring degrees. But he was a member of the royal college of surgeons, and

consequently had the same standing in the profession as Pott, John Hunter, Cooper and Abernethy.

"Such was his character in London as an anatomist, that it was distinctly understood he was to succeed the late Mr. Else in the anatomical lectureship at St. Thomas's and Guy's hospitals, the most lucrative situation of the kind, perhaps, in the world. In what manner Mr. Cline obtained the place, I need not mention—it is sufficient to state that it was not from any suspicion of superiority over his competitor. It was impossible for Mr. Baynham to be distinguished as a surgeon in England, as his labours were too much confined to the dissecting room. There, however, he was pre-eminent, being unquestionably the best practical anatomist of his day, in Great Britain. The best preparations in the museum of Messrs. Cline and Cooper, were made by him: one particularly of the female breast, it is supposed, has never been equalled. His name is mentioned in anatomical works as the discoverer of the vascularity of the *rete mucosum*. He supposed that the vessels, which he was the first to inject, existed in a healthy state of that membrane—but in this, he was perhaps mistaken, at least I have never seen them demonstrated, except where there was some cutaneous disease.

"You will find a paper of Mr. Baynham's in the 1st vol. of the Medical and Surgical Journal and Review, printed in New-York. It contains the account of an operation which he was the first, I believe, to perform successfully. The operation it is true, is not a difficult one—but it required considerable professional skill, to detect the true nature of so obscure a disease, and to devise the proper remedy. You are as well acquainted as myself with his general character as a surgeon. One remark I will add, which is, that Dr. Physick and Mr. Baynham are the only persons, whom I know, in America, that have really improved the surgical profession."

We are indebted, for the preceding biographical sketch, to an accomplished scholar, not of the profession, who had long the confidence and intimacy of Mr. Baynham. Writing, however, chiefly from recollection of facts delivered in conversation by his deceased friend, he has committed some inaccuracies, which, though not materially affecting the fidelity of the narrative, seem to require correction. This is done in the following statement, which is furnished by a near relative of Mr. Baynham, from authentic documents in his possession, and is entitled to the fullest credibility. EDITOR.

Mr. Baynham went to complete his education in London in 1769, where he became a student at St. Thomas's Hospital. Here he soon acquired the notice of Mr. Else, the professor of anatomy, which ripened into a reciprocal attachment, that continued through life. Under his direction, he became particularly attached to the study of anatomy and surgery, and by application and perseverance, he soon acquired a complete knowledge of both these departments of science. He was remarkable for unwearied and minute attention and diligence, in every thing he attempted, and thereby ensured success, if at all attainable.

As a proof of his early anatomical knowledge, he was employed in 1772, by the professor of anatomy at Cambridge, to dissect and prepare the subjects for his lectures, as appears by a letter from him to Mr. Else, dated at Cambridge, 12th April, 1772, of which the following is a copy:

"SIR—I beg leave to return you thanks for being instrumental in prevailing on Mr. Baynham to assist me, as his services have been entirely satisfactory, and his private behaviour very amiable and engaging.

"I am, sir, your very humble servant,

"CHARLES COLLIGNON,
Professor of Anatomy in the University of Cambridge.

"To Mr. Joseph Else, Surgeon to St. Thomas's Hospital,
and Professor of Anatomy in London."

Mr. Baynham continued to assist the professor at Cambridge in his dissections, &c. for several winters, and in the

remaining part of the year, was a partner with Mr. Slater, an eminent surgeon of Margate, which was a very profitable and agreeable connexion, as appears from some observations in Mr. Baynham's own writing now before me. In this situation he received a letter from Mr. Else, of which the following is a copy, from the original, now before me:

"Dear Sir—I am not well satisfied with Mr. Cline's manner of managing the dissections, and as he either will not, or cannot, make any considerable number of preparations—and as he is just married, I apprehend he will be less assiduous in future. There are several who are very expert in dissections, and would be willingly employed in his office, but there is none of whom I have so good an opinion as you. If, therefore, it could be contrived that you could be with me in the winter, for this purpose, to our mutual advantage, I should be glad. Hearing that you are shortly to be in town, I write to you, that if this matter is palatable to you, you may revolve it in your mind.

"I am, sir, your most obedient servant,

"Joseph Else.

"To Mr. Baynham, Surgeon, Margate, Kent."

When this proposal was made known to Mr. Slater, to induce Mr. Baynham to continue, he offered him an equal share in the partnership, but readily consented to his accepting a proposition so flattering, and apparently so advantageous. Accordingly he engaged with Mr. Else on the following terms: He was to superintend the anatomical theatre and dissecting room, prepare the bodies for his public demonstrations, make preparations for the museum, and to instruct his pupils in the arts of dissecting, injecting, making anatomical preparations, &c., at a salary of eighty and ninety pounds the two first years, and one hundred pounds a year for five succeeding years—at the expiration of which (having qualified himself in the interim for the office) Mr. Else was to relinquish to him the professor's chair, or to take him as joint professor, on equal terms, as he (Mr. Baynham) might choose. Having returned to St. Tho-

mas's, he pursued his occupation with unremitting assiduity, and thereby acquired that minute knowledge of anatomy, for which he was so justly celebrated. He prepared for the museum more than three times the number of rare and valuable preparations which he found there, in the space of five years—at the end of which time Mr. Else died suddenly, of an apoplexy, and without making any preparation for him as his successor. Mr. Baynham was even an entire stranger to the Governors of the Hospital, in whom was vested the power of appointing a successor to the late professor. His reputation as an anatomist and surgeon, however, was so well established, that when the election was made, his opponent obtained the appointment by one vote only, and that because two of Mr. Baynham's friends were absent. Mr. Else bequeathed to him the museum, by his will, at the price of six hundred pounds, which he afterwards sold to Mr. Cline, who obtained the appointment, for eight hundred pounds—reserving to himself some very beautiful and valuable preparations, which were on hand, and unfinished at the time, and which, with others, he had occasionally made for himself, were sold for one hundred guineas, to Mr. Blizard, professor of anatomy in the London Hospital—among these was a very fine preparation of a testis, beautifully injected, which satisfactorily decided the long disputed question between Else, Pott, Hunter, and others, whether in the operation of hydrocele by caustic, as performed by Else, the cure was effected by adhesion, or by a total destruction of the tunica vaginalis (as asserted by Else) in favour of adhesion. In this case the caustic was applied in his ordinary way, by Mr. Else, on a man in the hospital; and at that period of the cure, when portions of the tunica vaginalis were thought to be coming away in sloughs at every dressing: the patient died from another disease. Mr. Baynham dissected out the parts, and injected them by the spermatic artery, with a very fine injection, coloured with vermilion. The tunica vaginalis was much thickened, but otherwise perfectly sound, whole and entire in every part, except where the caustic had acted. On its

internal surface, as well as on the external surface of the tunica albuginea, where it is reflected over the body of the testis, there appeared a regular and beautiful inflammatory blush of new as well as old vessels, clearly demonstrating the actual existence of what Mr. Hunter termed adhesive inflammation, and establishing, conjunctly with the appearance of soundness in the tunica vaginalis, beyond a doubt, the fact, that the cure is performed by adhesion, and not by the destruction of the sac. It was, probably, about this time, when engaged in making a preparation of an exostosis, that he injected and demonstrated a fine vascular membrane on the surface of the cutis, immediately under the rete mucosum, separate and distinct from the cutis, and capable of separation from it throughout its whole extent. It has been said this existed only in the diseased limb on which he operated, but that was not his opinion. I believe he made similar preparations from other subjects, though I cannot positively say so, not having heard of the objection in his life time. He has told me, however, that Dr. William Hunter was satisfied of the fact, from the preparations which were made and showed to him, expressing his surprise, by saying, "what have we been doing so long that we never observed this before?" Mr. Cruikshank was then about to publish his observations on the absorbents, and proposed to attach an account of this discovery to his book, which was agreed to, and in that book is the only notice of it I have seen, except one, apparently taken from that source, in Wistar's Anatomy. The account given by Cruikshank is a gross misrepresentation, and an attempt to confound this with that part of the cutis he had been operating upon, to discover the the seat of the variolous pustules.

He presented a copy of this work to Mr. Baynham, which has on its margin various remarks in his (Mr. Baynham's) writing, expressing his indignation against the author for so doing, and clearly marking what he ought to have published. This, with his other books, was left to his nephew, Dr. Walter New, of the United States' Navy—and he promised me to send it to Dr. Wistar, that he might

know the truth. He gave me that book to read many years ago, that I might understand the subject, which, when I had done, I observed to him, that it explained to me the seat and appearance of erysipelatous inflammation, the cure of it by blistering, and the probability that scarifications through this membrane would arrest the progress of erysipelas, and account for the progress of superficial gangrene being stopt by a blister applied above it—this membrane being the seat of those diseases—to which he replied, if he had an opportunity, such as he had in London,he could soon decide the point. What is the situation of this membrane in the negroes? They are not subject to erysipelas (I have never seen or heard of any who had it); they are not subject to the itch and yellow fever.—Perhaps this membrane may not exist among them—yet they are as easily blistered as Europeans, and according to this notion, the inflammation of this membrane produces the blister. On the 7th day of June, 1781, Mr. Baynham became a member of the Company of Surgeons of London (which is to the surgeon what the degree of M. D. is to the physician) and commenced the practice of Surgery in London, and probably continued to do so while he remained in that city.

MEDICAL AND PHILOSOPHICAL INTELLIGENCE.

Phrenological Society of Philadelphia.

Though of recent formation, this Society embraces a number of the respectable and intelligent gentlemen of the city, and if we may judge from the wide field of investigation which it opens, will doubtless be of considerable utility, by leading to a more precise and observing study of the physical and moral nature of man.

Assuming as the basis of its inquiries, the great physiological truth, that all the phenomena of animated nature are displayed through organization in general, and that all the intellectual phenomena are evidenced by means of the brain in particular, the Society, in admitting the plurality of mental faculties, and corresponding cerebral organs, proposes to pursue "the study of the operations of mind as depending on proportional development of the brain." With this view, it will of course call in Comparative Anatomy and Natural History to its aid, by observing the coincidence between the various instincts of animals and their cerebral organization and development—and when furnished with illustrations from this quarter, will extend the examination to all the varieties of our own species, and see how far national and individual character may have received their first impress by peculiarity of cerebral structure.

It would, perhaps, be premature to speak of the proposed practical application of the Society's labour, to a nicer discrimination between feelings, and propensities, and intellect, so as to direct, with a prospect of success, that great moral engine, Education: and also, by a classification of primitive faculties, enable us to note more accurately their modifications, constituting individual character, and their disease daction, producing insanity.

The Officers of the Society are as follows:

President,
Philip Syng Physick, *M. D.*

Vice Presidents,
William E. Horner, *M. D.* | Clement C. Biddle, Esq.

Curators,
Wm. P. C. Barton, *M. D.* | Aaron B. Tucker, *M. D.*

Corresponding Secretary,
John Bell, *M. D.*
Recording Secretary,
Benjamin Coates, *M. D.*
Treasurer,
Joseph Cabot, Esq.

ANATOMY.

One of the most striking appearances which occasionally present themselves to the anatomist, is the transposition of all, or of some, of the viscera. Two instances of this deviation from the ordinary arrangement of parts have recently occurred: the one to Dr. Campbell, in the case of a child six weeks old, in the abdominal cavity of which were found only the liver and gall-bladder, the cardiac portion of the stomach, the termination of the colon, and the kidneys, covered with the peritoneum; while the cavity of the thorax contained the rest of the stomach, the small intestines, and the remainder of the colon, with the spleen, pancreas, and great omentum:—and the other case to Mr. Desruelles, of Paris, in a French soldier, in whom the transposition of the thoracic and abdominal viscera was general. Malformations of another kind are not unfrequently observed; and Mr. Windsor has given the description of a fetus, in which the urinary bladder was found attached, throughout its whole length, to the abdominal parietes, the attachment extending for the space of six inches, and as far as the umbilicus. The author of the present Essay had, not long ago, an opportunity of dissecting the body of a child, having a spina bifida, in which the bladder presented exactly the same appearances as those described by Mr. Windsor; a circumstance which gave to the abdomen the character of ascites during life. The child lived three weeks.

Two curious papers on anatomical subjects, by Mr. Magendie, are inserted in the last Number of his Physiological Journal. In the one, which he has illustrated with two plates, Mr. Magendie gives a description of the organ destined to tighten or relax the membrane of the tympanum and the ossiculi of the auditory apparatus, in man and mammiferous animals; and, in the second paper, he has detailed the anatomical appearances of a dog, born with one eye only, and without a mouth. One of the singular facts connected with the latter examination is the total absence of the optic nerve from the eye, notwithstanding which the retina, which has hitherto been considered as the expansion of that nerve, was found *in situ*, and perfectly formed.

There is a very curious memoir, in the Number of the Journal Complementaire for August, by a Mr. Gerdy, on the organization of the heart, which deserves consideration. The subjects treated at full length in this paper are—I. The albugineous tissue of the heart; II. The carneous tissue of the same organ. The former, according to Mr. Gerby, consists of—1, auricular zones; 2, the tendons of the valvular columns; 3, arterial zones; 4, the fringed margins of the aortic and pulmonary artery at their origin; 5, the tendons of the sigmoid valves of those arteries. The latter, or the carneous tissue, is different in the ventricles and in the auricles. The author describes them both very minutely, and then proceeds to the enumeration of several other very interesting subjects connected with the structure of the heart.

A case of a man having two distinct noses has been mentioned by Dr. Bidault de Villiers, who twice examined the individual presenting that deformity, at different times, very particularly, and has clearly described the phenomenon in the tenth volume of the Journal Complementaire.

A new division of the animal tissues in the human body has been proposed by Professor Mayer, of the Prussian University of the Lower Rhine, under the name of *Histologia*. The professor distinguishes—1, the lamellated tissue, to which he assigns ten distinctive characters; 2, the cellular fibrous tissue; 3, the fibrous tissue; 4, the cartilaginous system; 5, the osseous system; 6, the glandular tissue; 7, the muscular fibre; and 8, the nervous tissue.—*Bibliotheque Germanique.*

PHYSIOLOGY.

The effects of herbaceous vegetable aliments on the stomach of man have been observed by Dr. Gaspard, during the calamitous famine which desolated several districts in France, in the year 1817. The herbaceous vegetables used on that occasion were wild-sorrel, nettles, succory, thistles, the tops of French beans, and the sprigs of young trees. These herbs were minced, and boiled in the same manner as spinnage; but, when they became too hard, the inhabitants expressed a thick juice from them, by straining them through a piece of linen. The uniform result of such a regimen, continued for a long time, was a general serous diathesis or universal anasarca; and, in six cases, which proved fatal, dissection showed that the stomach and intestines were extremely contracted.

A subject which has often perplexed the ingenuity of physiologists, is that of the various sympathies acting on the organs of the human economy. So many are the morbid phenomena dependent on these sympathies, that the practical physician has not unfrequently been baffled in his expectations and judgment respecting diseases under his treatment. One of the principal difficulties inherent to this question, is the ascertaining of the positive existence of what has been called sympathy, as an active agent, in any of the phenomena of life, whether morbid or otherwise, under consideration. The difficulty, in fact, consists in being acquainted with the semiology of sympathies. This very difficulty is precisely what Dr. Gelcen has endeavoured to remove, by his observations contained in the eleventh volume of the Journal Complementaire; to which we request the attention of our readers. Mr. Caffin, a naturalist, at Orleans, has treated the same subject in a recent paper inserted in the Journal général de Medicine, for September, 1821.

SURGERY.

While physicians, by means of repeated trials, are endeavouring to establish the claims of iodine to the cure of bronchocele, surgeons proceed in the removal of that deformity according to professor Quadri's method of treating it with the seton. A curious paper, containing much information relative to this subject, and a successful case operated upon in the Quadrian manner, by Mr. A. C. Hutchison, will be read with great satisfaction.

The checking of the stream of circulation in the carotid by ligature, in cases of aneurism of that artery, is now performed with as little hesitation as any other surgical operation of more frequent occurrence. Although terminating in death, the case related by Mr. Coates, in which he tied the carotid artery affected by an aneurismal tumor of the largest kind, will be perused and referred to with interest and advantage.

The employment of ustion in certain diseases is becoming very frequent on the continent. Cases in which this powerful therapeutic measure has been resorted to with success, are to be met with frequently in the German and French Journals. Mr. Priou, of Nantes, and Dr. Desgranges, of Lyons, have added to the evidence already before us on this point. A case of cerebral affection, one of articular rheumatism, a third of epigastralgia, a fourth of submammary pain with dyspnœa, another of obstinate cephalalgia, and a foul ulcer, have all been conduct-

ed to a salutary termination, by these practitioners, by the application of fire. Mr. Priou has entered into some instructive inquiries regarding the history and various modes of practising this surgical operation.

Tracheotomy has recently been performed successfully in three cases. In the one by Dr. Klein, of Stuttgardt, for the removal of a bean; in the other, by a gentleman, whose name we could not catch, when the paper was read at the Medico-Chirurgical Society, in which a pebble, of the size and figure of a bean, was extracted from the trachea. In both cases, fresh evidence has been brought forward of the comparative insensibility of the trachea *proper*, when contrasted with the exquisite irritability of the .laynx In a third case, related by Mr. Porter, the operation was successfully resorted to for the cure of cynanche laryngea.

Sir A. Cooper has successfully extirpated a large adipose tumor, situated midway between the umbilicus and ensiform cartilage. When removed, it measured one yard and a quarter in circumference, and eighteen inches around its neck. It weighed about thirty-seven pounds ten ounces.

Nasal Polypi.—Count Pietro Moscati, of Milan, has invented an ingenious instrument for applying a ligature to polypi of nose that hang pendulous in the throat, which is entirely free from every inconvenience belonging to the instruments now in use for that operation.—*Giornale di Fisica & di Chimica.*

THEORY AND PRACTICE OF MEDICINE, AND MATERIA MEDICA.

In France, a Neapolitan physician has shown, with the assistance of appropriate quotations and much erudition, that the doctrine which Broussais endeavours to promulgate is taken, almost verbatim, from Baglivi, the illustrious Italian physician, who flourished at the commencement of the last century, and practised in Rome. This assigning as a cause of fever a topical affection, particularly of the stomach, is so strongly marked in Baglivi's writings, that we are almost tempted to consider Dr. Broussais as a mere plagiarist. To those who have read the summary account which we have given of that gentleman's doctrine, it will be needless to point out the almost *literal* similarity which exists between his promulgated principles and those of Baglivi, contained in the following quotation.

"Pluries vidi, dari febres nonullas in ipso principio graves

statim, statim periculosas, et periculosis etiam stipatas symptomatibus. Has vulgo vocat malignas, idest à venenifero productas humore: cujus imaginarii et commentitii veneni de causa, innumera statim præcribunt spirituosa, aromatica, alexipharmaca, calida, volatilia, et mille id genus inter se pugnantia remedia; quibus quod præcavere volunt periculum advocant et augent.

"Ipse enim, ut vera fatear, quæ diligenti observationi et matura didici meditatione, a duabus potissimum causis, malignas has febres pendere observavi inflammatione viscerum, et ab apparatu pravorum, crudorumque humorum in primis viis, &c."

And, farther on, when speaking of the paroxysm of fever, Baglivi describes its symptoms, one would imagine he was reading the tenets of the modern *Ecole Physio-Pathologique.* "Statim apparere videbis linguæ ariditatem, pulsus exiguos, extremorum frigus, anxietates et alia id genus, quæ *malignam febrim* (*ataxique* of Pinel,) denotarent; sed revera non *sunt malignitatis effectus,* (Broussais attacks precisely in the same language the author of the Nosographie Philosophique,) verum enim stomachi ab exaltato humore *irritati,* lacessiti, et afflicti; qua cessante irratione, et stomachi indignatione compositâ præfata cessant accidentia."

Even where Broussais piques himself on absolute and indivisible originality,—namely, in having strongly inculcated the necessity of avoiding strong purgatives in visceral inflammation, and of relying on bland laxatives, diluted acids, and enemas, we find him copying the very words of the Italian physician we have just been quoting, who first descanted on the same points, with equal force. "Fuge nimium remediorum copiam (exclaims Baglivi;) fuge purgantia tanquam pestem; si tamen ingens fuerit pravorum humorum in primis viis apparatus, qui morbum producat et foveat; fomentis et clysteribus minuendus; et si hæc non fuerint satis, potio aliqua tamarindata leniens danda, dummodo tamen gravis ardor, et extremæ inflammationis rabies, contrarium non requirant. . . . . In febribus ardentibus sal prunellæ specificum est. . . . . Aliquando utor nitro perlato, interdum vero antimoniato cum hordei decoctione."

The French physicians have of late directed much of their attention to the treatment of intermittent fevers by means of a new remedy, obtained from the bark of cinchona, combined with sulphuric acid. We allude to the sulphate of quinine; respecting the superiority of which over common bark in those diseases, we have seen numerous public attestations. This fact seems to afford the *rationale* of the effects said to be produced by a mixture of cinchona and sulphuric acid, which English

practitioners are occasionally in the habit of prescribing. Mr. Morson, chemist, of Fleet market, has detailed an easy method of obtaining the sulphate of quinine, in the last Number of the Monthly Repository.—*ib.*

The Case of Tetanus successfully treated in the York Military Hospital, at Chelsea, by M. A. Burnester, Esq. is very interesting. It occurred in a young man, in consequence of a slight injury to the index finger of the left hand. The symptoms were not of the most acute character, and he always retained the faculty of deglutition. He was bled very largely upon the accession of the disease; and mercury internally and externally, with warm bathing and diaphoretics, was prescribed. He recovered perfectly.

Mr. B. also makes mention of a case of tetanus, that was removed from the supervention of gangrene in the injured part.

Cholera Morbus at Samarang.—About a month ago, a very malignant disorder manifested itself among the native population at Samarang. The Javanese, Malays, and Chinese, who were attacked with it, died, for the most part, after twelve hours' indisposition; many fell down dead suddenly after a very short indisposition. This extraordinary mortality excited the attention of the faculty, and convinced them that the cholera morbus had reached the island. It soon attacked the Christian population; and, especially, was very destructive among the military. The terror and confusion became general, and were soon followed by the flight of the Europeans to the mountains and cooler districts. On some days, when the thermometer rose to 95 or 100 degrees, from 160 to 180 persons died at Samarang, as well islanders as Europeans and their descendants.

The general distribution of good medicines, and the cooler weather, have already checked this disorder. However, of a company of European hussars of eighty men, twenty-five have been buried; and, of five superior officers at Samarang, two majors, English and Depré.

For persons who have not enervated themselves by the use of strong liquors,—for those who have a good constitution and a cheerful disposition,—this disorder is not more formidable than an ordinary fever, provided they daily use the proper remedies. The poor Javanese, who wanted the inclination or the ability, have suffered the most.

The sickness broke out here a few days after it appeared at Samarang; it is, however, of a less malignant description. Of sixty Europeans who were attacked, sixteen have died. We have reason to hope that the cool weather, and the approaching rice-harvest, will wholly put an end to the sickness. Many

persons chiefly ascribe it to the bad food which the Javanese of the lower classes have been obliged to use for some months.—*Private letter from Souraucasta, in Java, May* 26, 1821.

The *Punica granatum* has been employed medicinally from the earliest ages, both in Egypt, Palestine, and in most Eastern countries wherein it is indigenous. We find it mentioned in the fourth book of Moses, and in the earliest Greek writers. Few authors on the materia medica, since those early periods, have been ignorant of its powerfully astringent, detergent, and tonic qualities. It has always been, on account of these, and now is, a common remedy among the Jews; and it still retains its character in popular practice in Syria, India, and other Mahometan countries. Its efficacy against tænia was not known in this country until 1807, when it was favourably spoken of in the Edinburgh Medical Journal for January of that year. The author of this present paper was induced to employ it in the removal of tape-worm, from having observed the decoction of the bark recommended in Dr. Fleming's catalogue of Indian medical plants and drugs, as an efficacious remedy in such cases. The communication is entirely confined to a detail of its success on several occasions of its trial. He neither furnishes us with any history of its employment in this capacity, nor reasons upon its mode of operation. The bark was prescribed by Mr. Breton, both in decoction and in powder. The former preparation was made from two ounces of the bark of the root, boiled in a pint and a half of water to three-fourths of a pint. Two ounces of this was given, when cold, every half hour, until four doses were taken. When the pulverized bark was exhibited, two scruples were prescribed every half hour for three hours. The tænia, according to the eight cases mentioned by Mr. Breton, was generally expelled four or five hours after the exhibition of the first dose of the remedy.

It would appear from the detail, that this medicine produces nausea, with giddiness, when pushed to its greatest dose; that it opens the bowels when given to that extent, and that it produces a noxious effect upon these parasitic animals. Although, in most of the instances related, the tænia was voided alive, yet, when it was plunged in a preparation of this substance, it writhed, and otherwise manifested great pain, and died within the space of five minutes.

Ligatures on the Extremities recommended in certain Diseases.—Dr. Franz, of Lissa, in Bohemia, details the case of a young woman, of seventeen years of age, who complained of a difficult, interrupted, and hurried respiration; accompanied

also with pain, shooting through the region of the heart, which prevented the possibility of a profound or lengthened inspiration. The countenance was very florid, and slightly tumefied; the pulse small, hard, and contracted; the artery felt stretched as a cord. Dr. F. prescribed tonics, with musk, and other antispasmodics; and the warm bath, with antispasmodic enemata, without any advantage. He afterwards had recourse to the application of ligatures upon the extremities, as they are recommended by J. P. Frank, in his chapter "*de spirandi difficultate,*" in nervous habits. "Ligatura supra cubitum et genu injecta mirabile hinc inde levamen attulit." They were accordingly applied above both knees, and above the left elbow. The relief produced was both instantaneous and complete. The respiration and the pulse became freer, softer, and more tranquil; the flush in the countenance disappeared, and she was enabled to leave her bed. The ligatures were applied during three hours, when the pain and swelling which they occasioned required their removal. As soon as they were relaxed (especially the one on the left arm,) the spasms of the chest returned. They were again applied, and the relief was as immediate as before. Those upon the lower extremities were afterwards removed, and that on the arm retained for some time, and relaxed at intervals. There was no return of the disease, and the patient rapidly recovered.

Dr. Franz found the same method successful in a similar case which subsequently came under his care; no other treatment was employed at the same time in either instance.

The symptoms detailed by Dr. Franz would appear, in our opinion, to have required blood-letting. Did not the ligatures, however, operate in a somewhat similar manner,—namely, by intercepting the return of blood to the heart, and thereby allowing that organ to overcome the distention of its cavities and neighbouring large vessels, which had previously existed, and which most probably had been the cause of the symptoms described? The relaxation of all the ligatures, if not gradually performed, would occasion, by the sudden and overwhelming flow of blood to the right side of the heart, a recurrence of the symptoms, which were merely the efforts of that organ to relieve itself, accompanied with those signs of irritation and spasm which are generally the concomitants of action induced in any part beyond its natural functions.

We would recommend the same method, as deserving of a trial in similar affections; at least in such cases wherein a pathology such as that just alluded to may be presumed to exist.

Prophylactic against Hydrophobia.—Cantharides, dissolved in vinegar, are usually employed, in Hungary, as a remedy

against the bite of mad dogs, in the case of both man and animals. This preparation is taken internally.—*Allgem. Mediz. Zeitung, April* 1821.

The notion that mercurial remedies pass from the stomach and from the skin into the circulation, and penetrate, equally, the solids of our animal economy, has been so prevalent, that Dr. Rhades, of Halle, must find it a matter of no little difficulty to support a contrary opinion. The physiologist has undertaken to repeat the experiments, once made by some of the Berlin professors, with a view to ascertain whether mercury, rubbed on the surface of the body of animals, passed into their blood; and, like them, he concludes, that neither mechanical agents nor chemical tests can detect the presence of the metal under the circumstances in question.

Four important questions in medeography have been discussed, among many others of minor interest, during the last half-year, in this country and on the continent. 1. The real action of hydrocyanic acid in certain diseases; 2, the effects of colchicum in diseases of excitement, and the superiority of small over larger doses of that medicine in the cure of the gouty paroxysm; 3, the virtue of cubebs in the treatment of gonorrhœa; 4, the influence of iodine on glandular and scrofulous disorders.

Each of these points has been the subject of much controversy and of many contradictory statements. While Dr. Magendie, in France, Professor Brera, in Italy, Dr. Behr and Professor Helmekin, in Germany, Dr. Granville, in England, and Dr. Nancrede, in America, openly avowed their conviction of the salutary effects of the prussic acid in pectoral complaints, founded upon several years' experience, and supported by numerous cases; Dr. Elliotson, Dr. Macleod, and a notable reviewer, who confesses that he has never tried that medicine, deny it any visible action on those diseases. Dr. Macleod's statement is a candid one, and, as far as it goes, conclusive of the fact that, in several cases of senile and chronic cough, the medicine in question failed to produce relief. This gentleman has also observed that the hydrocyanic acid has the property of exciting salivation; a fact worthy of consideration, and which has been confirmed by the observations of the author of the present essay.

On the subject of colchicum, Mr. Bampfield, one of the surgeons of the Royal Metropolitan Infirmary for Sick Children, has written a paper in support of the practice of administering small doses of colchicum, and of the specific virtues of that medicine in subduing the paroxysm of gout; which has drawn from Dr. Scudamore a reply, in defence of his particular doctrine and mode of administration of that medicine.

The property which cubebs are supposed to possess in speedily curing gonorrhœa, has been variously debated; but the subject is entirely confined to the profession in this country. The evidence, on the whole, seems favourable to the medicine.

Not so, however, with regard to the use of iodine, first proposed by Dr. Coindet, of Geneva, and since used, pretty extensively, in Germany, France, and England. Dr. Coindet will scarcely admit any case of failure; while others of his fellow practitioners and countrymen not only proclaim such failures, but complain of its deleterious effects, so as to induce some of the Canton governments to forbid its use, except under certain restrictions. Dr. Del Carro, of Vienna, on the contrary, says that neither failures nor accidents of any kind have happened, to his knowledge, in about one hundred cases which have been treated with iodine; while, in France, opinions are yet balanced as to the real properties of that substance on the human frame. In this country, no decisive result on this subject has been obtained; or, if it has, it has not been publicly stated. Sir Andrew Halliday, domestic physician to the Duke of Clarence, has indeed announced that, in three cases of scrofulous tumors, in which he prescribed the tincture of iodine, that medicine proved successful; and an anonymous correspondent, in the same Journal, has borne testimony to the efficacy of that remedy, though not to the extent of Sir Andrew; but we have had an opportunity of hearing the details of an experiment, made by two physicians of great reputation and extensive practice, which go to prove iodine to be an inert medicine in the complaints for which it has been recommended. In this conflict of opinions, a singular circumstance is to be remarked, namely, that a substance, admitted, on all hands, to possess high powers of excitement, should be recommended in such a class of disorders as *scrofula*. Are not scrofulous diseases produced, and are they not always accompanied, more or less, by inflammatory action? In conclusion, we ought not to lose sight, that the violent effects of iodine on the animal economy rest on the authority of men of high character; and that Dr. Nordhof, of Aubone, and Dr. Golis, of Vienna, urge the profession to be very cautious in the administration of that substance.

The use of oil of croton, in constipation of the bowels, has also been a subject of attention during the preceding six months. It is known that a single drop of this oil produces copious, and sometimes violent, purging. This medicine, however, is by no means new. Murray, in his Apparatus Medicaminum, has given the botanical description of the plant from the seeds of which the oil is obtained; and Rumphius has detailed, pretty extensively, the virtues of both the grain and the oil.

A fresh instance has been brought forward, by Mr. Thomson,

of Whitchurch, to prove that we are not always correct in blindly following the recorded maxims of old, respecting the proper doses of medicines to be administered. This gentleman has related a case of paraplegia, attended by anasarca, in which the latter symptom was quickly removed by means of *four and twenty* grain doses of gamboge at a time.

CHEMISTRY, &c.

The new compounds of sulphur and cyanogene, mentioned by Berzelius;—the combinations of subsulphate of alumine and potash, examined by Riffault;—the analysis of verdigris, by Mr. Phillips;—the formation of alcohol by fluoboric gas, by Mr. Desfosses, of Besançon;—the new combination of oxides with chlorine, iodine, and cyanogene, by Mr. Granville;—the experiments of Mr. Farraday on the new compounds of chlorine and carbon, and on a new compound of iodine, carbon, and hydrogen;—are a few of the many subjects of interest in chemical science, that have been debated during the last half-year. To these may be added the classical paper of Dr. Henry, of Manchester, on olefiant gas; in which there are many stubborn facts and experiments, militating against some of the propositions advanced by Mr. Brande, who had previously treated the same subject. The paper of Mr. John Dalton, on the analysis of mineral waters, deserves also the utmost consideration.

By far the most curious and important paper in chemistry, to be found in the foreign journals of the last half-year, is that of Mr. Serullas, on the alloys of potassium, and the existence of arsenic in the antimonial preparation used in medicine. We shall only mention one fact contained in the memoir in question, relative to the decomposition of tartar emetic by the action of fire. Mr. Serullas, by exposing that triple salt in a covered crucible to a red heat, has obtained an alloy of potassium and antimony, which burns vividly when exposed to the air, pyrophorus-like, and becomes oxidized. We are surprised that this paper has not yet attracted the notice of some of our scientific contemporaries.

Scientific and analytical Pharmacy has been much neglected of late years in England. In France, Germany, and Italy, on the contrary, (but particularly in the two former countries,) the subject continues to be most zealously attended to; and the results are such, that numberless vegetables have been analyzed, new medical preparations proposed, and a spirit of research manifested, which does infinite credit to the individuals interested in those investigations. It is sufficient to name Pelletier,

Caventon, Henry, Robiquet, Boullay, Braconnot, Berard, Cadet Gassicourt, Virey, and Vogel, all of them *pharmaciens*, to be satisfied that in analytical pharmacy, there are fewer persons of equal claims to public consideration.

The separation of the active principles from the different species of cinchona bark, by Messrs. Pelletier and Caventon, and the preparation of the different salts resulting from the combination of those bases with acids, form an epoch in analytical pharmacy. We have detailed these discoveries, as they were published in France, in several Numbers of our Journal. The analysis of pepper, by Mr. Pelletier, is also deserving of consideration. From the pen of Mr. Henry, the principal director of the general laboratory of the Parisian hospitals, we read, with pleasure, a memoir on the colouring matter of saffron; in which, besides the process of separating that principle from the plant, the author has detailed its specific properties, and a mode of obtaining the volatile oil contained in the saffron. The application of the facts brought forward by Mr. Henry, on this occasion, to the preparation of different tinctures in which saffron is present, must be obvious; particularly with regard to what is called the compound wine of opium, or the laudanum of Sydenham. In the Bulletin de la Société d'Emulation for September last, Mr. Desfosses has announced the presence of a narcotic principle in the *solanum nigrum;* an analysis of which he has given, with the process for obtaining the principle itself, which he has named *solanine.* This new vegetable basis, when pure, is in the state of a perfectly white powder, resembling somewhat the cholesterine in its aspect. It belongs to the class of alkalies, and forms an indistinct compound, not crystallizable with different acids. Its bitter taste is more fully developed by the acetic than by any other acid. It possesses highly narcotic powers on the animal economy, producing great somnolency, the evacuation of much glary and mucous matter from the intestines, and sickness at the stomach. Recent experiments by Mr. Vogel have not only confirmed the fact, that the expressed oil of bitter almonds owed its poisoning quality to the presence of prussic acid; but have led also to the mode of completely freeing that oil from the obnoxious principle in question: notwithstanding which circumstance, Mr. Vogel has found that the oil continued to possess highly deleterious properties, which seem essentially inherent to the oil itself, and independent of the hydrocyanic acid.

Various statements have been given, by different authors, of the action of water on magnesia; the sum of which, however, goes to consider that earth as insoluble in water. Dr. Fyfe, a lecturer on chemistry in Edinburgh, has, however, proved, by

direct experiments, that magnesia is soluble in water; that, like lime, it is more soluble in cold than boiling water, (a singular fact, for which we are indebted to Dalton;) and that the quantity of that earth taken up by the former, when compared to that dissolved by the latter, is as 5760 to 36000.

Both the French and the Americans have endeavoured to support a claim of priority in the application of steam engines to navigation; but a fact has been ascertained by, and is to be forthwith published in a work of, Mr. Partington, of the London Institution, which sets the disputed point at rest. From a patent, the specification of which is placed at the beginning of a rare tract in the British Museum, it appears that Jonathan Halls expressly stated, in 1736, that he should apply the steam engine for the conveyance of vessels. The work in question is illustrated by an engraved plate, representing a steam-boat towing a large vessel; and is accompanied by an account of the apparatus employed for that purpose.

M. le Chevalier Yelin, a learned Bavarian, discovered, some time ago, that needles of steel become magnetic when placed in a glass tube surrounded with a metallic spiral, and when electrical sparks, or the charge of a Leyden battery, were transmitted along the spiral. When the spiral was turned from left to right, and the sparks taken from the positive conductor, the end of the needle which points to it becomes a south pole, and the other the north pole, and *vice versa*. If a third needle, reckoning from its middle, is surrounded in a spiral manner with waxed taffetas, then the poles appear at the points where the spiral begins and ends. If, instead of a spiral wire, a rod of metal is extended along the glass tube, the steel needle, placed within, becomes feebly magnetic after several strong electrical discharges. The poles of a magnetic needle were entirely reversed by several electrical discharges along the spiral.—*New M. Mag.*

Magnetism.—The *Prussian State Gazette* mentions a highly important discovery, which Dr. Seebeck had communicated to the Academy of Sciences at Berlin, in three different sittings. It was on the magnetic properties inherent in all metals and many earths, (and not in iron only as was supposed,) according to the difference of the degrees of heat. This discovery, it is added, opens an entirely new field in this department of natural philosophy, which may lead to interesting results with respect to hot springs, connected with the observations made bythe Inspector of Mines (M. Von Trebra) and others, relative to the

progressive increase of warmth in mines in proportion to their depths. According to M. Von Trebra's observations, the heat at the depth of 150 feet below the surface of the earth is one degree; at 300 feet deep, two degrees; at 600 feet four degrees, &c.

MIDWIFERY.

A Case of a Separation of a Portion of the Uterus during Labour. By P. N. Scott, Esq.

"The portion separated contained in the os uteri, and an irregular part of the cervix surrounding it." We know of only another instance of this kind upon record. That also happened during severe labour, and took place nearly in a similar manner. The head of the child conveyed before it the "os and cervix uteri, torn from the vagina and the body of this organ." The same writer considered the accident of possible occurrence from the unskilful use of instruments.

Few instances of posthumous parturition have ever been recorded. Dr. Schenk, counsellor to the king of Prussia, and physician at Siegen, attended a lady who died of chronic bronchitis, in the first week of the ninth month of her pregnancy. On the arrival of her medical attendants, who were summoned from some distance to her, they found that the patient had been dead about three hours, and that no movement of the child could be discovered through the abdominal parietes. The Cæsarean operation was instantly proposed, but peremptorily rejected by the friends. The body of the deceased was left a whole day, undisturbed, on the bed: on the following day the clothes were changed, preparatory to her being buried. Nothing remarkable was observed on that occasion; but when, on the third day, the body was uncovered for the purpose of its being placed in the coffin, a dead male child was observed lying between the legs of the deceased; the placenta hanging partially out of the vagina. Dr. Schenk very properly regrets that, when the Cæsarean operation was objected to, he did not proceed to ascertain how far it would have been possible to deliver the woman in the usual manner.—*Journal Complémentaire.*

Practical facts in midwifery have been somewhat more frequently detailed in the public journals, and in other publications. The most extraordinary one of this kind is the case recorded by Dr. M'Keever, of ruptured vagina during parturition, so as to admit the escape of a yard and a half of the

intestinal tube; which, after passing through the gangrenous process, came away without destroying the life of the patient, who has since been afflicted with an artificial anus in the vagina. The child had been delivered with the crotchet, the operation lasting two hours! We recollect being present, about two years ago, at the dissection of a poor woman, who died immediately after being delivered with the forceps, in whom an extensive laceration (it was called spontaneous rupture,) of the vagina was found between the bladder and uterus, which had evidently destroyed the patient's life.

Mr. Scott, of Norwich, a very sensible and expert practitioner in midwifery, has related the case of a woman, who, during a severe labour, lost a circular portion of the uterus, which was exhibited at one of the meetings of the Medico-Chirurgical Society. On examination, *per vaginam*, about three weeks after delivery, Mr. Scott found a continuous cavity, without any distinction between the vagina and uterus. The patient recovered.

On the subject of ruptures of the uterus, Mr. Purton has communicated a paper to the Editor of this Journal, in which he relates the case of a woman who died undelivered of her tenth child, in consequence of a rupture of the uterus, through which the fœtus escaped into the cavity of the abdomen. Mr. Purton was not allowed to have recourse to the Cæsarean operation, either before or after death. A case has been related by Mr. Bailey, where the whole of the placenta was expelled before the child. Mr. Bailey, very properly, and without any loss of time, delivered the woman by turning the child. A fresh instance of what has been called spontaneous evolution of the fœtus is mentioned, in the same journal, as having occurred in the Infirmary of the parish of St. George's.

BOTANY.

The tree which produces the caoutchouc, or elastic gum, has again been described by Mr. Bringier, who found it growing in the region of the Mississippi, on the Arkanzas, and Red River. A plant, which dissolves in water, has been mentioned in our last Number. It is found in the South of France, in the form of a green and membranaceous envelope filled with a species of jelly, containing a number of elongated filaments.

A remarkable plant, of the order fungi, has been found growing in a solution of submuriate of ammonia. Dr. Fleming has communicated an account of this curious discovery to Dr. Brewster; and has accompanied his description with a drawing of the plant itself.

Mr. Saussure, some time ago, announced that the alkalies and saline substances, met with in vegetables, were derived from the soil in which they were planted. Messrs. Schrader and Braconnot, who repeated many of the experiments of Saussure, and contrived others, came precisely to a contrary conclusion ;—namely, that the greater part of the saline principles to be found in vegetables were the produce of vegetation, and were not absorbed by the plants from the soil. Mr. Lassaigne, to set this question at rest, planted several seeds in powder of sulphur, which were kept moist with distilled water. The grains germinated, and the plants grew to the height of six centimetres, surmounted by several leaves. On analyzing these, after inciseration, the same proportion of phosphate and carbonate of lime were found in them which were given by the same weight of the ungerminated grains.

More accurate descriptions of the varieties of cicuta have been published by Dr. Chevalier, than those which are to be found in the generality of works on botany. We beg the attention of editors of Dispensatories and Pharmacopœiæ to this interesting subject.

LITERARY INTELLIGENCE.

Dr. Armstrong will shortly publish a General View of the Nature and Treatment of Acute and Chronic Diseases. In this work Dr. Armstrong will show, from facts which he has collected since his residence in London, that what is called malaria, or marsh effluvium, is the primary source of typhus fever, and that this disease has three types, viz. the intermittent, the remittent, and the continued form, which pass and repass into each other, according to circumstances which will be pointed out. The question respecting the contagious or non-contagious nature of typhus will be discussed through a strict reference to facts. In this work, also, Dr. Armstrong will develop an extensive and important intercourse which exists between the stomach and the heart, and this intercourse will be pathologically and practically applied to chronic diseases in particular. Dr. Armstrong will, at the same time, adduce a body of evidence to show the great efficacy of full doses of opium immediately after venesection in acute inflammation of the bowels, and some other inflammatory affections.

Class I.—Authors who consider Yellow Fever an imported and contagious disease :—

Arejula, Joh. Eman. Darstellung des Ansteckenden Gelben Fiebers, in Malaga. Wien, 1804. In Spain.

Bally, M. sur le Typhus Amérique, ou Fièvre Jaune. Paris. In America.

Batt, William, Alcuni Detagli sulla Febre Gialla Contagiosa. Geneva, 1804. In Spain.

Berthé, J. N., Précis Historique de la Maladie qui a régné dans l'Andalousie, en 1800, &c. Mont. 1802. In Spain.

B. Progetto, sull' Origine, Natura, e Carattere della Peste, dei Contagi, e della Febbre Gialle. Lucca, 1304. In Spain.

Blane, Sir Gilbert. See the various writings of this very experienced physician. In West Indies.

Caizerques, Mémoires sur la Contagion de la Fièvre Jaune. America.

Chisholm, Charles, Essay on the Malignant Pestilential Fever, &c. London, 1801. In West Indies.

Currie, William, Treatise on the Synochus Icteroides, &c. Philadelphia, 1795. In United States.

Dalmas, Récherches Historiques et Médicales sur la Fièvre Jaune. Paris, 1805. In Spain.

Des Portes, Histoire des Maladies de St. Domingue. In West Indies.

D'Oyarvide, de lo Vomito Negro. Havan. 1801. West Indies.

Fellowes, Sir J., on the Pestilential Disorder of Andalusia. London. In Spain.

Lind, on Hot Climates. In West Indies and Africa.

Moreau de Jonnés, Monographie sur la Fièvre Jaune. Paris, 1820. In Spain.

Pym, William, Observations on Bulam Fever, &c. In Spain.

Reid, Diseases of the Army, &c. America.

Salgado, J. D., Refleciones acerca de la Epidemia que regna en Cadiz, &c. Mad. 1800. In Spain.

Class II.—Authors who consider Yellow Fever to be entirely local in its origin, non-contagious, and to depend upon an epidemic constitution of the atmosphere, when it assumes its more malignant and devastating characters: in short, to be essentially a non-contagious disease:—

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Bancroft, on Yellow Fever. In West Indies.

Barker. In American Medical Repository, Vol. VI. In United States.

Browne, Sam., Treatise on the Yellow Fever, &c. Boston, 1798. In United States.

Burnett, on Mediterranean Fever.

Cassan. In Journal Général de Médecine, p. 221, t. xxi.

Comstock. In New York Medical Repository, Vol. VI., No. 11, Art. 2. In United States.

Clarke, James, Treatise on the Yellow Fever, as it appeared in the island of Dominica, in the years 1793–96. London, 1797. In West Indies.

Davidge, J. B., Treatise on the Autumnal Endemial Epidemic, &c. Balt. 1798. In United States.

Devéze J., Traité de la Fièvre Jaune. Paris, 1820. In West Indies and United States.

Dickson. In 13th Vol. of Edinburgh Medical Journal. In West Indies.

Doughty, on the Nature and Treatment of Yellow Fever. In Spain.

Gilbert, N. P., Histoire Médicale de l'Armée Française à St. Domingue. Paris, 1803. In West Indies.

Hillary, on the diseases of Barbadoes.

Hunter, J., Observations on the Diseases of the Army in Jamaica.

Jackson, Robert. See the various writings of this truly eminent physician. In West Indies and Spain.

Jefferson. In United States.

Keutsch. In Nordischem Archiv. IV. B. 2. St. p. 83.

Lacoste, sur la Fièvre regnante en Livorno. Liv. 1805. In Spain.

Langerman J. G., Uber das Fieber, &c. Hof. 1805.

M'Arthur. In Dr. Johnson's Work on the Diseases of Tropical Climates. In West Indies.

M'Lean, on Pestilential Diseases.

Miller, Edward, Report on the Malignant Disease of New York, 1805.

Moore on the Domestic Origin of the Pestilential Sickness at Baltimore. 1800. In United States.

Mosely, on the Diseases of Tropical Climates. In West Indies.

Musgrave. In Medico-Chirurgical Transactions, Vol. IX. In West Indies.

O'Halloran, on the Fever at Cadiz, &c. Lond. 1821.

Ramsay, Facts concerning the Yellow Fever at Charleston. 1800.

Robertson, Arch. Observations on the Fever, &c. of New Orleans. In Dr. Johnson's Work, 2d Edit.

Rush, Benj. See the papers published upon the subject by this physician, in the Transactions of various learned Societies. In United States.

Savarési, A. M., sur la Fièvre Jaune en Générale, &c. In West Indies.

Selden. New York Medical Repository, Vol. IV. N. 4.

Trotter, Medicina Nautica, &c. In West Indies.

Valentin, Louis, Traité de la Fièvre Jaune. Paris, 1803. In America.

Whitehead. In American Medical Repository, Vol. IV. Num. 4. In United States.

Winterbottom's Account of Sierra Leone. In Africa.

Writer in Journal of Foreign Medicine, No. VIII. In Africa.

And Drs. Fergusson, Muttlebury, Denmark, Veitch, Vance, and Sheppard, are of the same opinion. In West Indies.

Class. III.—Authors who consider Yellow Fever local or indigenous in its origin, but capable of assuming a contagious character, under certain favourable circumstances:—

Eymann, de Typho Icterode Ind. Occident. Zeitung. 1801.

Hosac, Alex., on the Yellow Fever of New York. 1797.

Humboldt, Essais Politiques sur le Mexique. Journal Général de Médecine, 1811.

Johnson, J., on Tropical Climates. London, 1821. In East Indies.

Le Blond T. B., Observations sur la Fièvre Jaune. Paris, 1805.

Nicol, on the Fevers of Seringapatam. In East Indies.

Palloni, Observations Médicales sur la Fièvre Jaune régnante en Livorne. 1800. In Spain.

Pugnet, Mémoires sur les Fièvres de Mauvais Caractère, &c. Lyon, 1804. Mediterranean and West Indies.

Romans, Natural History of Florida.

Much of the preceding intelligence, we have extracted from a very interesting review, in a late number of the Medical and Physical Journal of London, of the progress of medicine for the last six months.—Editor.

UNIVERSITY OF PENNSYLVANIA.

At a Public Commencement held in the Hall of the University of Pennsylvania, April 4th, 1822, the following gentlemen received the degree of Doctor of Medicine:

MAINE.

John Hubbard, on Absorption.

MASSACHUSETTS.

Jonas Green, on Necrosis.

NEW JERSEY.

John Marshal Paul, on the Effects of Contusion on the Bladder.

Charles Garrison, on Cholera Morbus.

Isaac S. Mulford, on the Connexion of Scrofula and Tubercular Consumption.

Randall H. Phillips, on the Cholera Infantum.

Wm. Bacon, on Cold Applications.

Matthew Pryor, on Pertussis.

John A. Elkington, on Anomalous Diseases of the Rectum, &c. &c.

Benjamin Fisler, jun., on Dysentery.

PENNSYLVANIA.

Daniel Theodore Coxe, on the Conversion of Diseases.

Edmund Lewis Dubarry, on Wounds of the Joints.

Thomas Hendry Ritchie, on Artificial Pupil.

J. Townsend Sharpless, on Typhoid Bilious Fever,

James Kitchen, jun., on Indigestion.

Samuel M. Fox, on Varicose Ulcers.

Benjamin Sandford, jun., on the Circulation of the Blood.

Nicholas B. Lane, on the Epidemic of Franklin County (Penn.) in 1821.

Samuel Barrington, on Arsenic.

Edward Young Howell, on Cholera Infantum.

Amos G. Mathias, on Trachitis.

Wm. Buskirk Hahn, on Chorea Sancti Viti.

John Heysham Gibbon, on the Causes and Prevention of Drunkenness.

Robert Milnor, on the Absorption of Medicines.

John Uhler, jun., on Intermittent.

Wm. Barnwell, jun., on Tropical Dysentery.

Robert May, on Ophthalmia.

Samuel M'Cleane, on Scrofula.
Henry Merivale Tucker, on Asthma Convulsivum.
Theodore Ashmead, on Tetanus.
Benjamin Ellis, on Marsh Effluvia.
John O. Wagener, on Hydrocephalus.
Samuel Jones, on the Fever of Philadelphia in 1820.

DELAWARE.

Thomas M. Stout, on Rheumatism.

MARYLAND.

Wm. P. Williams, on Croup.
Robert Aloysius Durkee, on De Cubeba in Gonorrhœa.

VIRGINIA.

George Smith, on Puerperal Convulsions.
Augustine Abernathy, on Cholera Infantum.
Benj. Henry Moyler, on Gastritis.
Solomon Mordecai, on Phthisis Pulmonalis.
Gilley Marion Lewis, on Melia Azedarach.
Hugh Holmes M'Guire, on Tetanus.
Landon B. Cabell, on Hydrothorax.
George W. Morris, on Gonorrhœa.
James Cook, on the Fever of Fredericksburg, in 1821.
John C. Taliaferro, on Rest and Position as Remedies.
Wm. W. Fennell, on the Causes of the Vacuity of Arteries.
John Ker, on Amenorrhœa.
Edmund Berkeley, on Dyspepsia.
Mann P. Nelson, on Cholera Infantum.
Francis A. Willson, on Hydrothorax.
Richard B. Perry, on Hydrocele.
Fontaine Watson, on Pneumonia.
George Lee, on Calculous Affections.
Southy S. Satchell, on Anasarca.
James Spencer, on Cynanche Maligna.
Alexander S. Strachan, on Hydrocele.
Francis Lightfoot Lee, on Hydrothorax.
Robert D. Starke, on Rheumatism.
Thomas P. Hoge, on Hæmorrhois.

NORTH CAROLINA.

Nathan Turner, on Pneumonia.
Thomas L. Carthy, on Lithiasis.
Philip H. Thomas, on Hæmorrhoids.
Thomas J. Faddis, on Cholera Infantum.
Levin B. Lane, on Cynanche Trachealis.
Major A. Willcox, on Hepatitis.

SOUTH CAROLINA.

John Haslett, on Inflammation.
Shubel Blanding, on Morbus Coxarius.
Francis Orlando Curtis, on Intermittent.
Dugald Patterson, on Acute Hepatitis.
Moses Quarles, on Syphiloid Disease.

GEORGIA.

John D. Swift, on Hemeralopia.
Alexander Jones, on Melia Azedarach.
John Jordan, on Dyspepsia.
Wm. Rainey, on Bilious Fever.

DISTRICT OF COLUMBIA.

Ezra Stiles Meigs, on Hypochondriasis.

GREAT BRITAIN.

George A. W. Oldmixon, on Nux Vomica.

JOHN REDMAN COXE,
Dean of the Medical Faculty.

TRANSYLVANIA UNIVERSITY.

William Reid, Ireland—Thesis on Generation.
James C. Cross, Lexington, Ky.—Cynanche Trachealis.
James P. Parker, Lexington, Ky.—Tabes Mesenterica.
Henry Geo. Doyle, Louisville—Prognosis.
Charles J. Walker, Richmond, Ky.—Dyspepsia.
Robert L. Wiley, Salem, Indiana—The non-contagiousness of Yellow Fever.
James W. Hoggatt, Nashville, Tenn.—Mania.
Henry Miller, jr. Glasgow, Ky.—An inquiry into the relation between the sanguiferous and nervous systems.
Robert Monroe Pearce, Mississippi—On the importance and associability of the human stomach, both in health and disease.
Thomas Hunt, Tennessee—Gonorrhœa Virulenta.
Langston Cooper, Boonville, Mo.—Chronic Inflammation of the Liver.
David Pointer, Halifax County, Va.—Dysentery.
G. F. H. Crockett, Delaware—Asthma.
Daniel T. Orr, Woodville, Miss.—Bathing in Febrile Affections.
Joseph N. Bybee, Harrodsburgh, Ky.—Greek Thesis on Intermittent Fever.
Thomas M. Wooldridge, Frankfort—Marsh Miasma.
Walter H. Drane, Russellville—Phthisis Pulmonalis.
James A. M'Pheeters, St. Louis—On the apoplectic state of the system as it occurs in a variety of diseases.

Daniel A. Million, Fayette County, Ky.—An examination into the causes of degeneration of the human species, and the practicability shown of restoring them to their former stature and vigour.

Elihu Kilpatrick, Mississippi—Bilious Fever.

Byran R. Young, Hardin County, Ky.—Dyspepsia, or a derangement of the stomach.

John W. Brite, New-Castle, Ky.—Phlegmasia Dolens.

William W. Baker, Lancaster, Ky.—Cynanche Trachealis.

Joel Miller, Lexington, Ky.—Secale Cornutum.

Edward C. Drane, New-Castle—Hydrocephalus.

William Dixon, Greenville, Miss.—Dysentery.

William A. Rogers, South Carolina—Dysentery.

J. B. Hopkins, Hopkinsville—Use and Abuse of Calomel.

William H. Price, Franklin County, Ky.—Modus Operandi of Medicines.

Pleasant H. Butler, East Tenn.—Uterine Hemorrhage.

Robert Montgomery, Summer Co. Tenn.—The Medical Topography of Sparta.

Thomas S. Francis, Lexington, Ky.—Typhus Fever.

Josiah Hale, Monticello, Miss.—Teres Lumbricoides.

John R. Abbay, Montgomery Co. Ky.—Cynanche Trachealis.

Thomas D. Singleton, Williamsburgh, S. Carolina—On the influence of intense Study.

James R. Black, Shelbyville, Ky.—Conception.

An honorary degree of M. D. on Walter Brashear, of Lexington, Ky.

W. H. RICHARDSON,
Dean Med. Faculty.

COLLEGE OF PHYSICIANS AND SURGEONS, CITY OF NEW-YORK.

NOVA SCOTIA.

Ebenezer Fitz-Harding, on the Placenta.

CONNECTICUT.

Rufus Blakeman, on Melenipolia.
Stephen Middlebrook, on Puerperal Fever.
Henry Palmer, on Dropsy.

NEW-YORK.

Samuel Borrowe, jun. on Iritis.
John Cadle, A. M. on Mortification.
John P. Emmet, on the Chemistry of Animated Matter.

Peter Forrester, A. B. ————————
Nathan S. Jarvis, on Angina Pectoris.
Ferdinand Ludlow, on Hydrophobia.
Robert P. Bracombe, on Menstruation.
Jotham W. Post, on Diseases of the Hip Joint.
David L. Rogers, on Carotid Aneurism.
Walter S. Smith, on Emetics in Hysteria and Epilepsy.
Lewis Traver, on Hepatitis.
Spencer Wood, A. B. on Hereditary Diseases.
Andrew Van Dyck, on the Topography and Diseases of Kinderhook.

NEW-JERSEY.

John S. Condit, A. B. on the Lachrymal Gland.
Lewis D. Ford, on Dysentery.
Thomas L. Smith, on Cataract.

VIRGINIA.

Horace Ames, on the Phlegmasiæ.

NORTH CAROLINA.

Bennet Boddie, on the Functions of the Skin.
Samuel Grier, on Hepatitis.
William H. Hunter, on Dyspepsia.

SOUTH CAROLINA.

William B. Ellerbe, on Urethrites.
Aaron Lopez, on the Importance of Nosology.
David I. Means, on the Momordica Elaterium.
Edmund C. Park, on Catarrhus Senilis.
William Parker, A. B. on the Indigenous Gentians of the United States.

GEORGIA.

Fitzgerald Bird, on the Sanguinaria Canadensis.

THE

PHILADELPHIA JOURNAL

OF THE

MEDICAL AND PHYSICAL SCIENCES.

ART. I. *On the Functions and Diseases of the Liver.* By JOHN STEARNS, M. D., President of the Medical Society of the State of New York, &c.

WHETHER we contemplate the LIVER as a gland, or as a viscus, and consider its magnitude, the utility of its secretion, and its extensive sympathetic connections, we must ascribe to it an importance superior to any other in the human body. In the opinion of the ancients, it possessed still higher claims to notice. It was the centre of their circulation and the great factory in which the blood was perfected. It is, therefore, highly important that the functions of this organ and its secretions, and the diseases to which they are incident, should be correctly understood.

Since the modern improvement in physiological science, the preparation and secretion of bile are believed to constitute the chief use of this gland. Its modus operandi has long been a subject of enquiry—but so minute and obscure are the operations of glandular secretions, that they will probably for ever elude any successful investigation. We can only trace the formation of bile to the venous blood, conveyed to the liver by a singular union of the veins of the chylopoietic and abdominal viscera, in a vessel denominated vena portarum. As this is the only in-

stance of the distribution of venous blood through a gland for the purpose of secretion, we necessarily impute to it the acquisition of new properties, during its passage through other glands and viscera, which renders it better qualified for the preparation of bile. The peculiar nature of these changes has hitherto eluded the researches of the chemist: neither have his experiments succeeded in deciding the long agitated question, Whether the fluids secreted by the glands are entirely new, or had previously existed in the blood.

The hepatic arterial blood has generally been considered as destined exclusively for the nourishment of the liver: but from the arterial nature of the splenic blood and the transmission of a branch of the cystic artery to the vena portæ, we infer that the arterial, with some modification, is an auxiliary to the venous in the formation of bile.

It is the common opinion of physiologists that the cystic bladder, and its duct, are passive in the reception of bile, and that it is impelled into the duct by the vis a tergo, retaining precisely the same qualities with the bile from which it was derived. Some late experiments, by Dr. Douglass, lead to a different conclusion, and illustrate several important phenomena not before explained. It appears from these experiments, that the cystic duct acts as an absorbent, and selects from the bile in the hepatic duct its most active qualities. From this time the hepatic and cystic bile become entirely distinct in their qualities, destination and use. The hepatic, deprived of its bitterness and pungency, by the absorption of the cystic duct, becomes a bland and harmless fluid. It is now, with perfect safety, regurgitated into the stomach, forms a chemical combination with the chyme, thereby constituting chyle for absorption, and precipitating the fæces for evacuation, and is thus conveyed into the circulation, to aid in the formation of new materials for the blood, and nourishment for the body.

The bitterness, pungency, and viscidity of the cystic bile, indicate qualities very different from the hepatic, and such

as are peculiarly calculated to purge the intestines—which is probably the only purpose it is designed to answer. Its cathartic properties are increased during its retention in the cystic bladder, by the absorption of its dilute parts, and consequent inspissation. Some peculiar irritation produced upon the internal coat of the intestines, or mouth of the common duct, by the passing chyme, constipated fæces, or any acrimonious fluid, induces a sudden discharge of cystic bile, to answer the exigencies of nature. But if, by any morbid action the cystic bile should be regurgitated into the stomach, a nausea, vomiting, and deadly oppression are instantly induced, which can be removed only by the total evacuation of a fluid so deleterious to that viscus. Effects still more destructive are produced when it is absorbed by the lacteals, and carried into the circulation. The great depression of the whole arterial system, the slow, languid, and oppressed pulse, the yellow skin, and, indeed, the whole train of symptoms that immediately succeed this absorption, are so characteristic of that type of fever denominated malignant, that I am induced to consider cystic bile as an exciting cause of this species of fever. This was the favourite opinion of Hippocrates, and of his contemporary philosophers, and had it been founded upon a knowledge of a circulation and a proper distinction between the cystic and hepatic bile, they might have erected a system that would have resisted the wreck of time and the sophistry of succeeding theorists. May not fever owe its different degrees of malignity to the difference in irritation which the gradations of intensity in the cystic bile produce on the arterial system? A judicious investigation of this subject, may shed light on the theory of fever. The deleterious qualities of the cystic bile are proved by the experiments of Dr. Munro, who caused the immediate death of an animal by injecting it into the stomach.

We may, therefore, safely infer that it never exists in the stomach or blood vessels, without producing symptoms of the most dangerous tendency, while we have satisfactory evidence that hepatic bile abounds in both. This dis-

tinction may elucidate the common opinion, so incompatible with daily experience, that bile is never found in the stomach without inducing disease.

When the liver is preternaturally excited, and the bile too rapidly secreted, to have its due proportion of active qualities absorbed by the cystic duct, and is thus conveyed into the circulation, it produces a febrile irritation, which can be cured only by restoring the liver to its healthy and natural secretions—but which may ultimately terminate in the ejection of bile imperfectly formed, and occasionally mixed with the dark portal blood. This is denominated black vomit, and has been frequently traced from the stomach through the biliary ducts to the pori biliarii, in the liver.

I am aware that the ordinary occurrence of jaundice, without fever, may be adduced in refutation of this theory. But when we consider the sympathy between the liver and its ducts, it is easy to perceive that the obstruction of the latter will naturally affect the secreting process of the former, and that the pungent and active qualities of the bile may thereby be converted into a morbid and inert secretion that will be absorbed and conveyed into the circulation with effects much less injurious to the system. The yellow aspect of the skin, in some bilious and malignant fevers, proceeds from an increased absorption of bile by the lacteals, and not from its obstruction in the ducts and consequent reversion into the blood, as in jaundice. The difference in the colour of the skin in these two diseases, and the appearance of bile in the intestines in the former, and not in the latter, afford strong grounds for this opinion.

The distinctive properties which I have assigned to the hepatic and cystic bile, obviate the difficulties in considering it of an excrementitious or secretory character. While the former enters the circulation, and in common with other secreted fluids, performs important uses—the latter, like other cathartics, is destined for excrementitious evacuation. This opinion is fortified by the yellow colour of the serum without any of the bitterness of the cystic

bile: and also by the nature, use, and properties of the bile in the fœtus. The latter is entirely hepatic and destined exclusively for the nourishment of the fœtus: And even admitting its gradual absorption in the gall bladder, there is nothing of that sympathetic intestinal irritation, necessary to invite its discharge. The changes which immediately succeed the birth, from respiration and the deglutition of new food, induce that species of irritation which procures the first discharge of that cystic bile, which is necessary to commence, and for ever after to perpetuate the peristaltic motion of the intestines. It is to the inadequacy of this irritation, or to its protracted operation, that the bile often becomes stagnant in the liver, producing, by its absorption, that icteric appearance, so common in early infancy. The experiments of Dr. Douglass have contributed much towards the definitive settlement of this question. He ascertained the secretion of bile, every twenty-four hours, to amount to twenty-nine ounces, and was never able to discover more than five ounces among the evacuations during the same time—consequently twenty-four ounces are daily absorbed and carried into the circulation.

The common opinion of physiologists, that the gall bladder is entirely passive, and that the bile is caused to flow by the distention of the stomach, after a full meal, may be included among those errors which the authority of names, and the prejudice of education, have conspired to sanction. Upon this principle, every distention of the stomach, by flatus or other causes, and every prone posture of the body would empty the gall bladder, as a washerwoman empties her bucket of water. Neither can I believe that the agitation excited by vomiting, or any other means, can contribute to the same effect. All prescriptions founded upon this principle alone, must therefore be fallacious and inert. To reduce these important operations to such mere mechanical principles, is derogatory to that wisdom which formed our bodies, and animated them with living souls. And it is a subject of no ordinary surprise, that this theory

should have been sanctioned by the learned physiologists of every age. A correct view of the economy of nature in the living body, will conduce to the subversion of these false notions, and to the substitution of a theory more rational and consistent.

The whole animated machinery of man combines a system of causes and effects, mutually connected and mutually dependent, and deriving its principle of action from a self-moving power within. We can, therefore, never explain the operation of a single isolated part, without reference to its relations and connections with the whole. It is with a view to this connection that we admit the existence of a principle denominated sympathy or association, which the liver with the stomach, and intestines with the gall bladder, possess in an eminent degree.—With a full knowledge of this fact, can we divest these viscera of vitality, and ascribe their organized motions, secretions, and the discharge of bile, to a mere mechanical operation of passive inanimate matter?

When the stomach has received its supply of food and commenced the process of digestion, the associate action of the liver is excited by their sympathetic connection, for the purpose of supplying bile, which is always proportioned in quantity to the action of the digestive organs. The same principle promotes the secretion of the gastric juice, when nourishment is taken into the stomach, and also causes the saliva to flow at the sight of desirable food. Bichat's experiments prove that the pylorus remains closed during digestion, and that when the chyme is discharged at this passage, the bile is at the same time poured out from the duct.

This subject might be pursued, and facts adduced in its support, from the associate operation of every organ in the human body. But the principle is too well known to need that extensive illustration of which it is susceptible, and which requires only to be applied to the stomach, liver, and their appendages, to render this theory of their respective operations, perfect and consistent. The liver is pow-

erfully excited, and the bile copiously secreted, by the continued exposure to heat. Hence the occurrence of autumnal bilious fevers is always proportioned to the intensity and duration of the heat of the preceding season.

Two important objects, as conducive to health, are attained by this increased discharge of bile:—the food is more rapidly digested, the fæces more speedily evacuated, and their putrescent tendency more effectually counteracted. Highly injurious effects would probably result from the long exposure of either to such intense heat, should their operations be subjected to the delay of colder regions. It is, therefore, necessary that the residents of tropical climates should regulate their ingesta and regimen, so as to promote the formation of new blood, sufficient to supply the loss occasioned by the increased secretion of bile. The stimulants usually resorted to in those climates increase this secretion, without augmenting the pabulum from which the blood is derived. The blood thence becomes impoverished, the bile imperfect and acrid, and fluxes, fevers, and an alarming train of morbid affections, immediately succeed.

This theory is proved by daily experience, and the diseases and premature deaths of those who migrate to tropical climates, and fall victims to their imprudence, soon after their arrival.

The indeterminate use of the spleen, and the tributary aid which it yields to the liver, in the formation of bile, render it necessary to bestow a few remarks upon this organ. The capacity of its blood vessels, the red colour of its blood, and its general aspect, gave origin to the ancient opinion, that the chief use of the spleen was to impart the red colour to the blood. But when the liver and spleen were found to be affected at the same time with similar diseases, and an unusual quantity of blood conveyed from the latter, by the splenic vein into the vena portarum, the former yielded to the more correct opinion, that the spleen was subservient to the liver in the formation of bile.

The late Professor Rush considered the spleen as mere-

ly designed to receive the superfluous blood, when its preternatural accumulation in the vessels might hazard the safety of other parts more sensible to its effects, and therefore denominated it the waste-gate of the system. Contemplating the anatomical structure of the spleen, the magnitude of its blood vessels, their tortuous and minute ramifications, and its occasional enlargement when distended with blood, without sustaining the least injury, we are inclined to adopt this opinion. Yet that this should constitute its only use, appears inconsistent with the laws of animal economy, the important connection of its blood vessels with those of the liver, the relative situation of these organs, their mutual sympathy, and the general opinion of physiologists. These considerations afford strong grounds to believe that the spleen becomes a reservoir for the occasional accumulation or diminution of blood in that viscus, according to the requisitions of nature, for the supply of bile. Every excitement of the liver, by the presence of food in the stomach, by contagion, by miasmata, or by any violent exercise, fills the spleen with blood, and produces a correspondent secretion of bile. If no reservoir of blood had been provided for such emergent calls, the secretion of bile would have been diminished at the very time when its copious discharges were most needed. Those stimuli, therefore, which increase the secretion of bile, also accelerate the circulation, and fill the spleen with blood. If this reservoir had been located in those organs which have other functions to perform, their operations must have been occasionally interrupted by an excess or defect of blood. The wisdom of the Creator is, therefore, conspicuous in providing an organ whose operations are exclusively destined to this end.

A few remarks will now be bestowed upon those organs in their diseased state, which are affected by the morbid condition of the liver, and which are associated with it by the influence of a mutual sympathy.

1st. The Spleen. The subserviency of the spleen to the liver, and their mutual connection, are further evinced

in the diseases which the morbid affection of one produces upon the other. Dissections shew that an indurated and contracted liver precedes an enlarged spleen. This has been explained by Morgagni and others, by ascribing to the contracted and unyielding vessels of the liver, an obstacle to the usual flow of blood from the spleen, while an increasing quantity continues to enter it by the splenic artery. An accumulation of blood and the consequent enlargement of that viscus, become the necessary result. The same author found an enlarged liver generally to accompany an enlarged spleen, which has been confirmed by more modern dissectors. It was a common opinion among the ancients, that a diseased liver always preceded dropsy, and that a morbid state of the spleen soon afterwards succeeded. Although modern dissections do not confirm the order of this affection, yet in every case of established ascitès, one of these viscera has been found diseased. It was for this reason that Albertini considered this species of dropsy incurable.

2d. The Stomach. This has been already alluded to in explaining the associate connection between digestion and the secretion of bile: and although imperfect digestion has generally been considered as the precursor of vitiated bile, there are strong reasons for believing that the latter is more generally the exciting cause of the former.

As the sensibility of the liver exceeds that of the stomach, it is consequently more liable to become affected by disease. Hence hepatitis is more frequent than gastritis; and also that peculiar depression of mind so strongly indicative of morbid biliary secretion, generally precedes an impaired state of the digestive organs. It is for the same reason that some writers have assigned to the liver the power of transferring diseases of the stomach to other parts which have more sensibility, and also to the minor glands, which derive their diseases from this, their parental gland, as children inherit the morbid taint of their ancestors.

3d. The Skin. The hepatic sympathy with the skin is conspicuous and extensive, and although well known to the

profession, has not received that due notice in the history and treatment of hepatic diseases which its importance requires. Erysipelas, a certain species of phlegmon on the extremities, various cutaneous eruptions, and a peculiar redness of the nose, are well known indications of a diseased liver, and of the reciprocal influence of this sympathy. It is further evinced by the same causes producing a cotemporaneous secretion of bile and of perspiration. This has been particularly noticed by Dr. Johnson, in the tropical climates of the east. The extreme heat of that climate so violently excited the minute vessels of the skin and liver, as to procure, at the same time, copious discharges of bile and perspiration. To obviate the debilitating effects, and to moderate the excess of these discharges, the Asiatic and the African are compelled to anoint their skin with oil. It is thereby defended against the excessive heat; and the immoderate flow of bile and of perspiration is equally restrained. This also prevents that torpor of the skin and liver, so common in those climates, which succeeds to their inordinate action, or to their subsequent exposure to wet and cold. By this torpor both secretions are diminished, and the bile accumulates in the liver, where it becomes vitiated by retention, and ultimately produces painful effects upon the alimentary canal.

Those who have indurated livers, sallow complexions, torpid bowels, and a paucity of bilious secretion, have also a dry, harsh, and contracted skin. Both of these organs are also similarly affected by the depressing passions of fear and grief.

In diabetes, chlorosis, mania, and dysentery, perspiration is always deficient, and the secretion of bile also evidently diminished. Doctors Balfour and Johnson confirm this opinion in dysentery, and add, that if these secretions can be restored and the plethora of the portal circle reduced, before the intestines are much injured, the cure will be completed. Contrary to common opinion, cholera affords another instance of diminished bile, and a dry skin. When the bile is checked and the vessels on the surface

constricted, in the incipient stage of this disease, if the efforts to vomit, which immediately ensue, do not succeed in restoring perspiration and the secretion of bile, death soon occurs. The deluges of bile, which burst forth when secretion is restored, shew the great plethora in the portal vessels which its temporary suppression had induced. The warm bath, stimulating cordials, opium, calomel, frictions and hot applications to the skin, cure cholera by restoring the hepatic and cutaneous secretions. And I have no hesitation to affirm that frequent ablutions, frictions, and other suitable purifications and incitements to the skin, will generally counteract a strong predisposition to hepatic diseases.

4th. The Mind. The influence reciprocated between the mind and the liver, is productive of well known and important effects. Violent anger affects the liver with spasm, and causes a regurgitation of bile into the stomach and immediate vomiting. Hepatic diseases are generally accompanied with a depression of spirits, a tædium vitæ, melancholy, hypochondriasis and a choleric disposition. It is, therefore, on a healthy state of this organ that our happiness in this life essentially depends. A cheerful disposition is the most effectual antidote to a diseased liver, and I can attest to the cure of one of the most inveterate and dangerous affections of this organ, by the uniform preservation of a remarkably cheerful mind, continued in opposition to the influence of the disease, by a succession of very cheering occurrences.

The idiopathic diseases of the liver may be reduced to the following organic affections.

I. Inflammation. This may affect the membranes or the substance of this gland, and is acute when it is confined to the hepatic arteries, or chronic, when it affects only the venous or secreting system. The first is often mistaken for an inflammatory affection of the chest, the stomach and contiguous muscles, and the latter for dyspepsy. This may terminate in resolution, scirrhus, suppuration or gangrene.

II. Tubercles. These consist of the following species:—

1st. Common tubercles, which extend through the whole substance of the liver, and are generally caused by intemperance.

2d. Large white tubercles, which constitute its scirrhosity.

3d. Soft brown tubercles, which are incorrectly denominated scrophulous.

4th. Scrophulous tumours, which nearly resemble the tubercles of the lungs.

III. A soft state of the substance of the liver, which John Hunter denominated interstitial absorption. It seldom affects the young, but is a common disease of advanced life.

IV. The induration of the liver. This is a very common disease.

V. Hydatids.

These are common, and when in the substance of the liver, are found enclosed in a cyst, but when on the surface, hang pendulous into the abdomen. They are said to contain animals imperfectly formed.

VI. Increased secretion of bile.

VII. Diminished secretion of bile.

VIII. Morbid secretion of imperfect bile.

The gall bladder is also liable to—

I. Inflammation. This often produces adhesion of its coats to the contiguous parts, but seldom ulceration.

II. Scirrhosity.

III. Ossification of its coats.

The hepatic and cystic ducts are also liable to become obliterated, and are often dilated by the passage of the gallstones.

The bile is diseased when it is imperfectly secreted, and thence becomes extremely acrid to the intestines, or is ejected from the stomach with the usual appearances of black vomit. Or when by long stagnation in the gall bladder it becomes green and viscid.

The symptoms and treatment of the diseases of the liver, are so minutely detailed and judiciously described by practical writers, that I am compelled by my present limits to refer to them, for a correct knowledge of the diagnostics and general mode of cure. I cannot, however, refrain from observing that calomel constitutes a very important remedy in most hepatic affections. It relieves torpor and inflammation, opens obstructions, and promotes the secretion of bile. Its successful operation is materially aided by the application of epispastics and mercurial ointment to the right hypochondriac region. The sympathy between the skin and liver before alluded to, designates the importance of these auxiliaries to the surface. When these remedies have succeeded in restoring the healthy action of the liver, and the consequent secretion of bile; the peristaltic motion of the intestines is immediately excited, the previous constipation of the bowels begins to yield, the evacuations become regular, and exhibit the appearance of bile in each discharge. With the inceptive appearance of these favourable effects, we discover an abatement of all the symptoms of the disease, with an increased secretion of bile and of perspiration.

These indications evince the necessity of a total change in the previous course of treatment. It must be obvious to every practitioner of experience, that a perseverance in the same remedies, would excite the liver into preternatural action, and thus accelerate the secretion of morbid bile, which by its acrid qualities would produce an obstinate diarrhœa that no remedies could restrain, while these stimulants were continued.

The skill of the physician is not more eminently displayed in the selection of his remedies than in the adaptation of suitable doses to the varying gradations of disease, or in his sagacity to discover the precise moment when to arrest the course of treatment. To persist in it beyond this period, however salutary and successful in the commencement, would constitute it the source of a new disease, that would baffle the skill of the physician, disappoint the

elated hopes of the patient, and produce despondency and death. Errors in practice from this cause have prejudiced the inconsiderate against some of the most valuable articles of the materia medica, which their ignorance and obstinacy have finally succeeded in consigning to oblivion.

Art. II. *Observations and Experiments on Absorption.*

By John Hubbard, M. D.

In a late number of Silliman's Journal, is contained a "Memoir on the mechanism of absorption in animals of red and warm blood, by M. Magendie of Paris." The results of the experiments there detailed, affect not only the mode of absorption, but the agents by which it is effected, and in their consequences lead to all the absurdities of the antiquated doctrines of transudation—though by him applied principally to the explanation of absorption. They consist mostly, in exposing isolated portions of the carotids and jugulars in living animals, while the blood flowed freely through them, to the contact of poisonous fluids, and in subjecting portions of dead vessels and membranes to a similar operation—and they go to prove that all fluids, whether poisonous or innocuous, readily permeate the coats (*parois*) of all the vessels of the living system, both great and small, venous and arterial. A proposition, so entirely opposed to all we know of the animal economy, could scarcely merit attention, though supported by the high authority of M. Magendie, were it not intimately connected with the doctrine of transudation, which is still defended by M. Richerand, and, more especially, were it not intended by Magendie himself to support his favourite doctrine of venous absorption.

The arteries and veins, so far as they are agents of direct circulation, and are subservient to each other, can in no other way be instrumental in absorption. Anatomists

have not demonstrated the origin of veins upon the surfaces of the great cavities, nor in the cellular structure of the system at large:—nor have vessels, other than the exhalents proceeding from the arteries, or terminating in them, and opening upon these surfaces, ever yet been demonstrated. On the contrary, all we know of anatomy proves directly the reverse. The whole system of vessels, the lacteals and lymphatics, whose peculiar function it is to absorb by their extremities, has been demonstrated to terminate in a few large trunks, which are discharged under peculiar mechanical advantages into the subclavian veins. Many anatomists and physiologists have, it is true, spoken of absorbents terminating in veins, of the radicals of veins, of the roots of the vena portæ, &c.—but their existence is entirely hypothetical. Hence some mode of action, other than by their mouths, is necessary to the support of venous absorption—hence too, M. Magendie has recurred to the more obvious and simple explanation by lateral orifices.

Before proceeding to a detail of our experiments, which lead to results directly opposite to those of M. Magendie, we shall state more explicitly some of the propositions he has deduced from his, and accompany them with some objections drawn from phenomena, constantly occurring in the living system, as well as from the structure of the arteries and veins themselves. His leading propositions are the following:

1st. That absorption is not a "*vital action*," but "depends on the capillary attraction of the vascular membranes, on the absorbed matter."

2d. "That all the blood vessels, whether arterial or venous, great or small, in dead bodies or in living ones, possess in their coats (*parois*) a physical property, by which we may perfectly explain, and to which we may reasonably refer, all the principal phenomena of absorption." The experiments upon which these propositions are principally built, have been already briefly noticed, but they will be more particularly considered in another place. Indepen-

dently of the absurdity of attempting to account for *vital* phenomena by a recurrence to *physical* laws, the inadequacy of capillary attraction to the explanation of absorption in general, as effected by capillary vessels, has been so satisfactorily demonstrated, as to render any remarks upon that subject entirely unnecessary. It has been shown, that this cannot be the moving power even in vegetable circulation *—hence *à fortiori* there is strong ground for the belief, that it cannot be concerned in the animal. The supposition, which by some has been resorted to, that the absorbed fluid is simply imbibed by this power, and afterwards propelled by other causes, involves the absurdity, that a tube, a portion, of the living system, is so constructed, that one part of it shall be entirely passive, while a contiguous portion shall be endued with all the properties of vitality—or, which is equally unphilosophical, that the very portion, which is discharging the office of an inanimate capillary tube, is at the same time acting with all the powers of a vital agent. Two actions, either of which would, alone, be adequate to the office, would, upon the latter supposition, concur in producing the same effect.

Having made these remarks upon capillary attraction, as applied to the system generally, we shall proceed to that modification of it, which is involved in the propositions above stated.

Admit the coats of all the vessels, large as well as small, to be constituted with organized pores, passing directly through them—admit these pores to act on the principle of capillary attraction, what would be the consequences? Capillary tubes draw fluids from masses to places where there may be a deficiency. Their tendency is to produce an equal distribution of fluids between the spaces connected by them. They may be compared to the conductors of a galvanic apparatus, which serve to produce an equilibrium between the extremities of the series, and, that equilibrium being restored, can circulate no more fluid, until

* Vide Thompson's Chemistry, Art. Veget. Chem.

the cause, first disturbing it, shall effect an accumulation at one extremity. In like manner, the vascular system, thus constituted, and replete with fluids, acted upon by the power above supposed, would be drained, until an equal distribution should be caused throughout the whole animal structure, and every portion made a confused mass of fluids and solids promiscuously intermingled—unless some restoring power recall the fluids to the vascular system, which power must be equivalent to absorption, an absorption too, other than capillary, because, as already stated, that power, from its well known laws, must cease to operate, as soon as an equal distribution occurs. Instead, therefore, of the fluids being carried into the circulating mass, the well known result of absorption, they must be carried from it, and absorption still remains to be explained.

Admit the reality of the operation of this power, on such a structure of the coats of the vessels, and of the membranes, for they must of necessity partake of the same structure, and we at once destroy the idea of the vascular system being the great reservoir of the animal fluids. We reduce this harmonious frame to the character of the sordid earth we tread upon, or of the lifeless sponge, the habitation of the puny reptile of the deep. Again: admit the truth of this theory, and secretion, exhalation, absorption, in short, every operation of the living system, where fluids are to be transmitted from one part to another contiguous, must be influenced by the same cause. The same cause, like satyr's breath, must produce the two opposite effects of transudation, or deposition, and of removing the fluids thus deposited—a supposition, involving the absurdity of one and the same structure, being formed with two distinct sets of pores or vessels, the one of which, by vital action, secretes and deposits its peculiar fluid, the other, by an action purely mechanical, removes that fluid, or, which is inconsistent with the well known rule of philosophizing, constituted with two sets of pores, both acting upon the same principle, i. e. capillary attraction, but producing the opposite effects of two currents, passing in opposite di-

rections. Take which horn of the dilemma you will, the argument is of equal force. In short, upon such an hypothesis, there could be no such disease as partial dropsy, no such disease as dropsy at all—Hydrothorax, ascites, every partial collection of water, would be immediately converted into anasarca, and this, passing to the lower extremities, would there form œdema, or, transuding through the cuticle, would cure itself at last.

Again, the structure of the arteries and veins affords additional proof, that they cannot absorb by lateral pores through their coats. So far from constituting them mere strainers, nature has carefully guarded against the extravasation of their contents, by supplying the vessels themselves with their nutrient principle, by instruments of life, the vasa vasorum, derived from a distant source, and cautiously distributed between their different coats, by passing in an oblique direction, instead of passing directly through their substance. The more effectually to guard against this accident, she has formed them with an internal coat, very smooth, dense, and firm, apparently impermeable to the most subtile fluid. She has even supplied them with lymphatics, whose mouths open, not only upon their internal surfaces, but into their substance, for the purposes, both of removing any effete matter, there deposited, and of interstitial absorption.*

All of these provisions must have been entirely unnecessary, were these vessels themselves endued with the power of absorption.

Besides, this doctrine supposes the absorbed fluids already elaborated, and is at direct variance with that nice discrimination in the different structures, of which we have evidence so abundant—a fact, of which M. Magendie seems fully aware, and one which he attempts very disingenuously to refute. It supposes the fluids already elaborated, because capillary attraction, being the result of that reagency, which exists between all ponderable bodies, limited in its operation to that of solids upon fluids, can have no agency

* Vide Cruikshank's Anatomy of the Absorbents, page 49.

in determining the character of those fluids, or in selecting them, when formed. All fluids must be indiscriminately taken up.

At this enlightened stage of physiological research, it can hardly be necessary to adduce facts in support of a discriminating faculty in the vascular and other structures. The effects of the different irrespirable gases, introduced into the trachea, of medicinal and other substances of a poisonous nature, upon the fauces, œsophagus, and stomach, of the contagion of lues, or any irritating matter, applied to the course of the lymphatics, in short the whole phenomena of health, as well as of disease, and particularly of inflammation, from whatever cause, are some of the innumerable instances, which might be adduced in support of this truth. Bichat, in treating of this subject, uses the following language: "Thus the larynx closes itself to all bodies but the air, which are accidentally introduced. Hence the excretories, though in contact with a number of various fluids on the mucous surfaces, which pass over or remain on these surfaces, are never penetrated by any of them. Hence, also, the open mouths of the lacteals in the intestines imbibe chyle only, and never absorb the fluids which are found mixed with it, fluids with which their sensibility is not in affinity."*

Having thus as briefly as was consistent with the object, stated our objections to the propositions already laid down, objections drawn from the phenomena of health and of disease, which are constantly occurring in the living system, as well as from the structure of the vessels themselves; having, we trust, shown, that these phenomena are totally inexplicable, by a recurrence, in part, or in whole, to any such physical operation, as supposed by M. Magendie; we shall, in the next place, proceed to the consideration of the experiments upon which M. Magendie builds his propositions, and follow them by a relation of our own, directed to the same purpose.

Those of M. Magendie, upon which he mostly relies,

* Vide Bichat on Life and Death.

were four in number on the living animal—for, as to those on dead vessels and membranes, they are entitled to no weight. Though not numerous, our experiments were sufficiently so to repeat his most important ones: nor have we simply confined ourselves to a repetition. We have varied them in some important respects, by instituting a second series of experiments, which, though more immediately directed to another object, have a very strong bearing upon the first series.

The following are M. Magendie's experiments.

A thick aqueous solution of alcoholic extract of nux vomica was applied to the jugular vein of a young dog, about six weeks old, "stripped carefully of all its appendages, especially of the *cellular substance*," and placed on a card for its whole length. Before the expiration of the fourth minute its effects were manifested, feebly at first, but soon with so much activity, that artificial respiration was resorted to in order to prevent the death of the animal. Great care was taken "that no portion of the poison should touch any part except the vein and the card, and that the current of blood in the interior should be unobstructed." On a full grown dog subjected to the same experiment, the same effects were produced, but from the greater *thickness* of his veins, not until after the tenth minute. Next, two full grown rabbits were subjected to the same experiments, on their carotids, but from the superior *thickness* of the coats of arteries, to those of veins, it was more than fifteen minutes before this poison could traverse the sides of their arteries. The application was then immediately suspended, notwithstanding which, one of the rabbits died. On opening the vessel the taste of the nux vomica was apparent on its inside.

The substances used in our experiments were the hydrocyanic, or prussic acid, and a *thick aqueous solution of alcoholic extract of nux vomica.* I was assisted in these performances by Dr. Henry Bond,* of this city, in whose

* Dr. Bond was one of the committee, appointed by the Academy of Medicine, last summer, for some physiological investigation, but as that

skill in practical anatomy, and in whose fidelity, the utmost confidence may be reposed. Drs. George, Colby, and Jewett, of New Hampshire, were also present.

Rabbits and kittens were the only animals employed.

Exp. 1st. To test the activity of the acid, a rabbit about two months old was used. Applied two drops of prussic acid to its tongue. In three fourths of a minute, falls on its side and lies motionless—in two and a half minutes, respiration quick and short—in four minutes, pupil contracts and dilates—in five minutes, heart felt to pulsate, but otherwise the animal appears dead.

Exp. 2d. Rabbit same age, laid bare the jugular vein, and separated it from the surrounding parts for near two inches—inserted a card beneath it, and kept the vein moist with the acid for twenty minutes without its producing the least effect. The hemorrhage during this experiment amounted to only a few drops, and a free circulation through the vein continued during the whole time.

Exp. 3d. Full grown rabbit. Placed a card under the jugular vein, as in the preceding experiment—applied the nux vomica. In thirteen minutes, struggling and some irregularity in the pulsation of the carotid—in nineteen minutes, seems strongly convulsed—in twenty minutes, respiration laborious and a rigidity of the muscles—at twenty-two minutes, let him up—in thirty minutes seems quite well. Here we might have been led into a gross mistake, by attributing to its being absorbed through the vein, effects which were undoubtedly owing to the operation of this poison in contact with other parts. But upon examining the card which was very thin, and, together with the vein, was kept constantly moistened, we found its under surface tasted strongly of the poison, the card being saturated with it. To this circumstance, undoubtedly, are attributable the slight effects produced. The animal was afterwards killed

committee never met to prosecute the object of their appointment, he offered me his assistance and the use of his anatomical room in the performance of my experiments.

with violent convulsions and apisthotonos, by the application of three drops of the same poison to the tongue.

Exp. 4th. Full grown rabbit. Put a piece of sheet lead under the jugular vein, and kept it constantly moist with the nux vomica for thirty minutes, without its producing the slightest effect. Six hours after, the animal was perfectly well, and was used in another experiment.

Exp. 5th. Full grown rabbit. A piece of sheet lead one inch wide was inserted under the carotid artery, without occasioning the least hemorrhage, and the artery kept wet with the nux vomica, for 30 minutes, without the slightest effects—let him up—he eats grass as usual. The poison was repeatedly dropped on the artery, and a piece of sponge thoroughly saturated with it laid on that portion which was over the lead. In this, as in all the preceding similar experiments, a free circulation continued through the vessel during the whole time.

To test the virtue of the acid used in experiment 2d, a second time, we tried the following: applied one drop to the tongue of a rabbit two months old. In one minute, respiration hurried, and convulsive motions—in one and three fourth minutes, rolls on his side and struggles—in two and a half minutes, respiration ceases—four minutes, gasps—four and a half minutes, dead. From the above experiments, we think ourselves fully entitled to the following conclusion; viz. That the *large* vessels do not "possess in their coats a physical property,"—"capillary attraction,"—by which fluids are carried directly into their cavities. As to the small vessels, the only *experiments* adduced by M. Magendie, to prove the existence of this property in them, were performed upon *dead matter*. On the contrary, the non-existence of this property, both in them and in the vascular membranes, has been proved by arguments, to us, conclusive and satisfactory. Should it be objected to these experiments, that the poison may have entered the cavities of the vessels without producing death, or any apparent symptoms of violence, it may be answered, that this very entrance was alone inferred by M. Magendie from such

symptoms. Besides, there can be no doubt, that death would be the immediate result of a very minute quantity of the prussic acid entering the blood vessels—a point which we had intended to test by experiment, but were unavoidably prevented. But we have the authority of Fontana, than which none can be higher, that such would be the result. "The venom of the Viper, the Ticunas, and the *Cherry-laurel*, when injected into the jugular vein, act instantly so as to occasion death, even when employed in very small quantities."*

Our second series of experiments has a very strong inferential bearing upon the result of the former—though they go more immediately to show in what manner the prussic acid and nux vomica (and other similar poisons) produce their effects upon the living system.

Exp. 6th. Kitten ten or fifteen days old. The acid was tested by applying one drop of it to the tongue. In one half minute, the animal falls prostrate—in seven minutes all appearances of life cease.

Exp. 7th. Kitten, same age. Lifted the skin from the upper part of the foot, and applied two drops of the acid—Waited twenty-two minutes, without observing any effects from it. We then lifted the skin on the inside of the thigh of the same kitten, near the body, and applied two drops of the acid. Symptoms the most violent followed for ninety minutes—but upon visiting him again three and a half hours after, he was recovered, with the exception of some tremor.

Exp. 8th. Kitten, same age. Same experiment was tried on the thigh, with the same result. Next morning the animal was entirely recovered.

In the two last experiments the lymphatics and large superficial veins were exposed, but with no hemorrhage.

Exp. 9th. Rabbit, about two months old, the same as employed in experiment 2d. Two drops of the acid were applied to the superficial wound of the neck, when fresh, so near the breast, as not to come in contact with any of

* Vide Fontana on Poison, vol. II. page 388.

the important nerves exposed in lifting the jugular vein. No effect was produced. Some time after, lifted a piece of skin on the inside of the thigh, near the body, and applied a drop of the acid with very little effect. At the end of the experiment the animal being released, leaped about with his usual activity, and without manifesting any signs of pain or debility. The loss of blood was very trivial. Next, laid bare the sciatic nerve, and applied one drop of the acid to it while lying on its connexion.—In one and a half minute, effects very manifest—in two minutes falls on its side—in five minutes, slight motion of the tail and tremor of the abdomen—in nine minutes, slight motion of the whiskers, but upon the application of the most delicate substances to the nose, no evidence of respiration is discovered.

Exp. 10th. Rabbit two months old. Laid bare the right par vagum and inserted a card beneath it—applied the acid to it repeatedly, so as to keep it constantly moist, but with great caution that it might not come in contact with other adjoining parts. The respiration was rendered deep and laborious, with occasional panting. At the end of eighteen minutes, the left par vagum was isolated in a similar manner, and the acid applied to both at the same time with no very considerable increase of effect.

The symptoms produced in this experiment, were probably caused by the violence done to the nerves in isolating them, for at the end of twenty minutes, when the cards were removed, they entirely ceased. Some time after, the same experiment was tried upon the sciatic nerve isolated in the same manner, but with no marked effect—then removed the card and applied several drops of the acid to the wound and nerve, *in situ*, by which the animal was thrown into violent struggles, with great difficulty of respiration, and finally at the end of thirteen minutes entirely destroyed.

Exp. 11th. Rabbit two months old. A similar experiment was performed upon the sciatic nerve placed upon a card in the same manner, and no effect followed. The card was then removed and three drops of the acid applied

to the wound by which the animal was destroyed in the same manner as in the preceding experiment.

Exp. 12th. Full grown rabbit, the same as used in experiment 4th. Applied three drops of the nux vomica to a wound made in the groin, so as to expose the crural nerve and lymphatics. Some small blood vessels, which were cut across, occasioned slight hemorrhage. In two minutes let him up, upon which he was immediately seized with violent convulsions and motions resembling apisthotonos, and in nine minutes died.

Exp. 13th. The same experiment performed upon the rabbit employed in experiment 5th, gave a similar result.

To the greater age and tenacity of life in the rabbits employed, are undoubtedly attributable some of the results apparently different from those obtained in similar experiments upon the kittens. They uniformly resisted the effect of similar quantities of poison with much greater obstinacy.

From a review of the whole, it will be seen that the prussic acid and nux vomica have been applied with impunity to large veins, to arteries, and to nerves, isolated from their connexions, as well as to large veins and lymphatics, *in situ*, unaccompanied by any large nerves—that when applied to the superficial absorbents and veins on the inside of the thigh and other parts in quantities sufficient under other circumstances to kill instantly, though violent symptoms in some cases followed, death was in no instance the result. But that when applied to a large nerve, *in situ*, immediate death has in every instance been the result—that the effects induced have been proportionate in their violence to the importance and extent of the nerves involved, and apparently affected by no other circumstance, except, as was in some cases suspected, by the division of small blood vessels.

Finally then, we think ourselves entitled to the following conclusion, viz. That the prussic acid and nux vomica, probably produce their effects through the medium of the nerves alone, and that they are not, as M. Magendie sup-

poses, "absorbed, or in other words, pass into the sanguiferous vessels, mix and circulate with the blood, and there produce their effects."

Having thus taken a general, though imperfect view of the most important propositions contained in M. Magendie's paper, we shall leave his numerous minor deductions from them to share their fate.

P. S. Since completing the above experiments, I have perused in the last number of this Journal, some experiments embraced in the report of the committee of the Academy of Medicine, performed "with coloured and chemical substances" upon the pleura, peritoneum, &c. They correspond in their general tenor with mine, and obviate the necessity under which I might otherwise have laboured of noticing certain experiments of a similar nature, upon the pleura, contained in M. Magendie's memoir.

ART. III. *Thoughts on the proximate cause of Fecundation in Vegetables.* By THOMAS NUTTALL.

THOUGH all attempts to trace the absolute origin of animal or vegetable life may truly be deemed presumptive, yet to examine and reflect upon the means which nature has employed to perpetuate her types of organized bodies, is a subject pre-eminently deserving the attention of the naturalist and the philosopher. Experiments and observations have indeed already been multiplied in relation to it, to a very considerable extent, and theories of different kinds have been framed and supported by men illustrious in philosophy and patient in research.

In botany we have seen a system arise and flourish with a success unparalleled, which had no other foundation for its classification than the sexual system believed to prevail throughout the vegetable kingdom.

The impregnation of the ovum, according to many successful experiments, has been attributed to the action of the pollen found in the anthers of flowers. The precise mode of this impregnation by the pollen, and its capacity under different circumstances for the purposes of nature, have been variously interpreted. Many of the disciples of Linnæus were led to believe that it actually furnished the embryon, as it did not begin to be visible in the pre-existing ovulæ, until a considerable period after inflorescence. Spallanzani's observations, however, on Basil,* in which the embryon, though infertile, was produced in the total absence of the anthers, which were carefully extracted, proves that its existence was independent of the pollen, though in its absence destitute of the vivifying principle.

In the Hemp, (*Cannabis sativa*,) he appears to have obtained perfect germinative seeds, independent of the access of pollen. But in the annual mercury (*mercurialis annua*) not a seed ripened in the total absence of the staminiferous individual, though some were perfected even when the individuals were removed to a considerable distance from each other. The interesting experiment of Gleditsch upon a date palm, which had grown eighty years in the Royal garden at Berlin, adds additional evidence to the importance of the pollen, as this plant was fertilized by its access, after it had been gathered nine days, and those flowers only produced seed on which it was sprinkled.

According to all the microscopic observations which have been made upon the pollen, its magnitude and form are various, perhaps, almost as much so as the genera of plants, were we in possession of glasses sufficiently powerful to ascertain its precise character. As nature has employed the most admirable economy in all her operations, it is only reasonable to suppose that such variations are intended to meet the exigencies of those organized bodies in which they exist. Delighting in variety, every thing has apportioned to it the means of perfecting its kind inde-

* Spallanzani's Dissertations, vol. II. (3.) p. 346. (Ed. Lond.)

pendent of climate and locality. Thus while the majority of the vegetable kingdom produce perfect or hermaphrodite flowers, in which, consequently, there is scarcely a possibility of barrenness, there are a few others in the Linnæan classes of monoica and dioica, in which the circumstances of fructification are widely different. In the latter of these classes, almost under every circumstance, actual contact for the purpose of impregnation is entirely proscribed, and accidents frequently separate the individuals of different sexes, remotely from each other, yet, under natural circumstances infertility is as uncommon in these tribes as in those which are hermaphrodite. Expedients have indeed been imagined by which this infertility might be obviated,—and certain insects have been impressed into the service of vegetables to convey the pollen of the male to the blossoms of the female. The uniform success of this experiment may, however, well be doubted, and more particularly, in such dioicous plants as the poplars and willows, whose flowers are evolved by the first return of spring, and long before the busy labours of the bees and mellivorous insects have commenced.

In the genus Vallisneria, so common in the tide waters of the Delaware, the male flowers never attain the surface of the water, the granules of pollen alone, bursting connection with the parent, rise and float on the surface of the water, expanding at the very instant they feel the impression of the air—and to how many accidents are these frail visitants exposed on an inconstant element before they may be floated into the contact of the styliferous flowers, which indeed I have often seen by thousands, without discovering in their vicinity a single flower of the opposite sex, after the most assiduous search, though all appear to be fertile—a remark which I have equally verified in the flowers of the *Udora*, a genus also nearly related to Vallisneria.

But to conclude as Spallanzani and Smellie have done, that the stamina are either partially or entirely useless in impregnation, is sceptical and unphilosophical—totally contradicted by prevailing matter of fact.—Not only is their necessity ascertained, but their employment in producing

hybrid varieties constantly practised by horticulturists and florists—and as at this day I conceive it unnecessary to defend a doctrine which has received the almost universal suffrage of naturalists, I shall now proceed to offer a few remarks upon the probable mode of impregnation in vegetables. From the experiments which have been made upon hemp and spinage by Spallanzani, and upon the *lychnis dioica* by Smellie, it must be sufficiently evident how active and recondite in certain circumstances is the process of fecundation. Botanists have indeed justly remarked the caprice of vegetable nature in the production of flowers—that in the number and the nature of their parts they are occasionally subjected to luxuriance or depauperation in such sort, that it can never be perfectly ascertained whether flowers supposed to be uniformly of different sexes may not conceal more or less of that hermaphrodite nature which is so much more prevalent in the vegetable kingdom. Notwithstanding the unequivocal character of the stamina, nothing is more common than their transformation into petals; indeed, in the petalostemoris, *five* of the *ten* filaments, which, according to the analogy of pafilionaceous order ought to produce anthers, support petals in their place. In many of those highly fragrant carnations and pinks, as well as double poppies, which we cultivate for ornament, seed is often produced in the total absence of the stamina, which are transformed into a multiplicity of petals. Indeed, if I mistake not, so close is the analogy of petals and stamina considered by the celebrated Decandolle, that he is inclined to consider them as analogous to so many enlarged filaments destitute of anthers, and as constituting no distinct and constant organs of the vegetable economy: but may they not as in the carnation, often, in the absence of the stamina into which they are liable to be transformed to a certain extent, answer the purposes of impregnation? It cannot be denied, that they frequently present similar sensible qualities, more particularly that of odour—and for what purpose to the plant is this odour evolved—this volatile quality, as transient as the existence of the stigma, coeval with

the perfection of that organ, and perishing with it? Nature has made nothing in vain; she has guarded against every contingence, and often, to the limited perception of man, dealt rather in superfluity than retrenched in parsimony.

What wonderful care has she not taken to perpetuate the principle of life, when, according to the experiments of Spallanzani, the spermatic fluid of the frog, though so vastly diluted in water as to be only as 1,064,777,777 to 1! was still prolific.

We have no just reason to conclude that the energy of vegetable vivification is at all inferior to that of animals. Nay, the very ambiguous experiments which have been made upon the bisexual specie almost stands as an argument in favor of the powerful and secret energies of the origin of vegetable life. How subtile indeed, must be that fluid or substance of whatever nature, necessary to elicit the life of the vegetable, which before it can possibly be felt in the *pre-existing* ovulæ, must traverse the *vascular* system of the style, and penetrate the umbilical funiculus! for, from the nature of the integuments which surround these ovulæ, as well as from their situation on the placenta, no other possible mode of impregnation can take place—in short no other connexion subsists between the vegetable parent and her embryon seeds. The source of life and of nourishment, therefore, in this class of organized beings appears to be the same.

Whether nature may have employed any similar means of impregnation in the animal kingdom, or whether it may be one of the many peculiarities of vegetable existence, is a subject beyond the scope of the present observations, and one too foreign and complicated for the feeble light of the author to elucidate. Nothing, indeed, as Smellie very justly objected, would appear more repugnant to reason and analogy, than even the suspicion of the *external* impregnation of the vegetable ovulæ. A substance of such tenuity, as we have every reason to suspect that which efficiently produces the impregnation of the vegetable, whether liquid or volatile, were it conveyed with ever such

certainty into the interior of the embryon fruit, a conclusion entirely irrational, would still find the admission of its influence opposed by those integuments or barriers of the seed destined for no other purpose than the protection of the embryon, when nature should expel it from the nourishment of its parent. On the other hand, no connection can be more direct and effective with the ovulæ than that which constantly subsists between the vessels of the stigma or *glandular* summit of the pistillum, and the funiculus of the ovum or embryon seed. We are not then to conclude with Smellie, that because external impregnation seemed impossible, the use of the pollen became nugatory, or that the pistillum had the power of impregnating itself! There is indeed, reason to suppose, that a system much more in unison with the ingenuity and harmony of nature, appears to prevail no less in the animal than the vegetable world.

The experiments of Spallanzani on the artificial impregnation of the frog, which takes place externally in every instance, cannot be adduced as any valid objection to the possibility of impregnation by the funiculus: we have grounds to suspect, as well in this case as in the above, that the most permeable part of the inclosed *fœtus* is that point which previous to their recent ejection connected the mother with her *inanimate* offspring. In this singular animal, every *oviform fœtus*, as soon as separated from its receptacle, becomes enveloped in a spherical mass of gelatinous matter destined for its protection, in such sort that it perishes without its access, and the impregnation thus becoming impracticable or imperfect within the body of the female. Nature without offering any real violence to her usual economy in those amphibious animals, and fish destined to produce a numerous brood, admits the possibility, in consequence of the necessity, of external impregnation.

The ensuing article was originally published in the form of a pamphlet. But as the edition was small, we are requested by the author to give it a place in this journal, to secure to it a more diffuse circulation. We are happy on this occasion to meet the wishes of so valuable a correspondent, and the more so, as we believe the article has been seen by very few of our readers.

EDITOR.

ART. IV. *An attempt to demonstrate, that the Bladder may be opened for the extraction of the Stone, by a posterior method of operating, more conveniently to the surgeon, and with much greater safety to the patient, than by any other method hitherto discovered.* By ELISHA NORTH, M. D. Author of a Treatise on Spotted Fever.

THIS subject may be illustrated by first showing, in detail, the manner in which the operation is to be performed, and exhibiting the advantages which may result—and then obviating such objections as may arise.

The reader is supposed to be well acquainted with the anatomical structure of the parts concerned in the operation of lithotomy, and with all the methods which have hitherto been contrived for performing the same—otherwise what is written may not be intelligible to him.

If the writer had the benefit of suitable plates, it would render both his and the reader's task more easy.

A table of convenient height and breadth is to be provided, and from the legs of the same let there be projections of wood at one end. These projections should form a right angle with the legs of the table, and may be about the length of the patient's legs—the distance from the uppermost surface of the table, at which these projections of wood should be attached to the legs of the same, may be ascertained by measuring the length of the patient's thigh.

The table and every thing else being ready, and the previous operation of introducing the long grooved staff finished, and the staff secured in its proper situation by the

hand of an assistant or other means, the patient may now be put on the table upon his belly, in the kneeling posture, his knees and legs resting upon the projections of wood above mentioned—his thighs should come in contact with the legs of the table—his thighs and legs may now be properly secured in this situation—the penis and staff should hang downwards, near to, and over the edge of the table. The patient's arms may be brought downwards in contact with the fore-legs of the table, and there secured. The fore-legs of the table should be shorter than the hind ones, so that the patient's body may be thrown into the position of the *inclined plane*. His head and face may rest upon a pillow.

If we have expressed ourselves so as to be understood, the reader will perceive, that the position is essentially the same as the one in common use for performing the operation of cutting the *fistula in ano*. And a moment's reflection will, we think, show, that the same position may be proper either for cutting into the bladder through the prostate gland, or for cutting into the rectum, as these parts lie in contact.

A suitable low seat will enable the assistant surgeon to manage the long staff. The operator will be conveniently seated posterior to the patient between his legs. He will now have all the parts on which he has to operate in a proper position for that purpose. He may then make the first or external incision precisely as it is at present done, by the best surgeons—and also slit up the membranous part of the urethra in the usual manner, cutting into the groove of the long staff. The common female grooved staff can easily be introduced into the bladder, taking the groove of the long staff for a guide. This last instrument may now be withdrawn, as the surgeon can have no further use for it.* Or if the surgeon dislikes the female staff he may

* We wish to observe that we are aware, that one who is much in the habit of performing the operation of lithotomy might prefer to carry in the common gorget *inverted* upon the groove in the long staff, without the introduction of the straight director. This such an operator might

furnish himself with a director made like the common grooved director in every surgeon's pocket-case, only a little longer and having a suitable beak at one end, to facilitate its introduction upon the groove in the long staff. What we insist upon is, that the groove in this director should be straight, as every one knows, who knows any thing on the subject, that it is much more convenient for the surgeon, and, we believe, much safer for the patient to carry forward any cutting instrument in a straight direction than in a curved one. It can hardly be necessary to mention that this almost straight director, (the beak part being a little curved,) should be introduced with an alternate kind of motion by which the urethra will be gently stretched from side to side, greatly facilitating the introduction of such an instrument. By turning the groove of this director to one side, the operator is enabled to make, with almost any kind of knife, the true lateral incision in the prostate gland and neck of the bladder. It may be done very well with the common gorget, inverted, the beak being ground off, provided he can use that instrument with his left hand.

The advantages obtained by introducing the straight director, are these—it brings the hands of the operator nearer to each other when he makes his incision into the bladder. This gives him more steadiness—he also gains the power of pressing down the bladder at a greater and safer distance from the rectum—it enables him to introduce a common scalpel, if he prefers that instrument to the gorget. The knife, however, should be of sufficient size to make the opening in the prostate gland and neck of the bladder, large enough to admit the finger, as the operator may want

easily do. *All we mean strenuously to contend for is, that our position of the patient ought to have the preference,* and that our directions are appropriate to the experienced operator.

We also think proper to remark that a dexterous lithotomist might, without the aid of our table, make a very simple business of this operation upon a resolute patient, simply by putting him upon his hands, elbows and knees upon a couch or some such piece of furniture, placing himself behind.

to carry in a smaller knife, turned flatwise upon his finger, with which he is to enlarge the wound by *lateralizing* the instrument. It may be proper to mention that it is contended of late that the wound in the prostate gland and neck of the bladder ought not to be too large, lest the *prostate fascia* might be injured.

The reader will now perceive that the parts proposed to be cut are the same, as the best operators have determined should be cut.

Our next object is to show that this mode of operating is preferable to any other. With this view we wish to suggest to the reader the following considerations :—It is well known, that the dread of inflicting pain and the want of suitable habits, owing to the infrequency of performing important operations, are the difficulties with which the surgeon has to contend.

The operations of the mechanic are so often repeated, that he easily acquires the necessary associations, constituting habit. This renders it easy for him to accomplish what he frequently has occasion to do. It is also well understood, that our hands and the association of the muscles connected with them, are such, that we can perform any motion downwards, with more facility, than the contrary way. We can also do a nice mechanical act better with our hands close to us, and near to each other and directly before our eyes, than at arm's length :—we also gain a mechanical advantage by sitting at our ease.

Now it appears to us that many of the foregoing principles in human nature are unnecessarily violated by performing the operation of lithotomy in the manner it has hitherto been done. The surgeon is required to make an orifice into a hollow vessel, for the bladder is nothing more, into or near its under side, and this vessel is so far posterior to the os pubis and os ischium, that these project in his way.* To make this orifice, he has to move his hands

* For the scientific reader will recollect that when a perpendicular is erected from the lower extremity of the os coxcyx, the bones of the pelvis laying upon a horizontal plane on the back or spine, that the portion of

upwards, and at times they are at a great and inconvenient distance from himself and from each other. He has also to shift his position in the midst of the operation, his hands, at the same instant, being confined by holding in contact two slippery instruments—and what is still worse, he has to carry in by the sense of feeling principally, that uncouth instrument, the gorget, upon the under side of a curved director—which movement is an inconvenient and unnatural one, besides being a critical one—yet this critical movement is to be performed with his hands at a great and inconvenient distance from himself and from each other. It is not so strange that the surgeon, who is called on to perform such a variety of motions, all of which are so contrary to his natural and usual associations and habits, and that too at *distant intervals*, should consider this the most important and difficult operation in surgery.

Notwithstanding philosophers and surgeons have chosen to perform the operation of cutting into the bladder upon its under side, (except in what is called the high operation,) for, say two thousand years, yet it is perfectly obvious, that were a common mechanic called on to make a similar opening into a hollow spherical body of wood or other substance, he would go to work with his tools upon its upper surface. Were he asked the reason why, he would say because he could do it more handily. He could not give the philosophical one, although he would perfectly well understand the fact.

Many of the foregoing difficulties will unquestionably be obviated by the method we propose. Besides other benefits are expected to result. The surgeon sits at his ease during the whole of the operation—he operates upon the upper

the tuberosity of the os ischium on which we sit, projects about three inches from this perpendicular. Now the opening in the bladder is made in the region of the perineum,—not in the region of the lower or inferior portion of the tuberosity of the os ischium. If the scientific surgeon will place the bones of the pelvis before his eye in the position which we contend is the proper one for the patient, who is to undergo the operation of lithotomy, and carefully attend to what we have written, we believe he will comprehend our meaning without the aid of plates.

surface of a spherical body—the parts on which he operates are near him—his motions are downwards—he sees distinctly all he does—he conveys in his knife upon a straight director—his hands are near himself, and near each other—his work is posterior to the os pubis and ischium; hence the danger of cutting the pudic artery is lessened—the rectum is also safe. All the motions of the surgeon are natural and easy—hence self-confidence and consequent steadiness must be the result.

The patient, as well as the surgeon, is more comfortably situated—for he has the power of closely embracing a solid substance during the moments of pain. The common position, as the celebrated John Bell has said, "is a horrid one in the extreme." The patient is put in the unnatural situation of a half circle upon his back—his hands and feet tied together, and the movements of one who is to inflict pain directly before his eyes.

We now proceed to obviate such objections as may arise. It may be said, and with truth, that none but an anatomist ought to attempt to perform this operation,—that he is in the practice of dissecting and contemplating the relative situation of the parts in the perineum, and within the pelvis, anteriorly—and that it would be difficult for him to break in upon his association of ideas on this subject. The answer is, teach the young anatomist to dissect these parts posteriorly, i. e. by turning the subject over on the belly, for this is the most convenient way for this dissection, as well as for cutting for the stone, and then his ideas will be correctly associated.

It may be said, that he who manages the long staff may not be so well accommodated. Admit this to be so, the part he has to act is a very unimportant one, compared with that which the operator has to perform, and whose convenience should have the preference in a case of this sort?

The introduction of the long staff in one position and opening the bladder in a different one, may displease the prejudices of some. As, however, the distress of the patient is not thereby increased, we think the objection of little importance.

It may be said that the stone could not be extracted in this way so easily. Now this, we think, cannot be true. If the operator wanted to tap a hollow vessel, which he could not turn over, to draw out a fluid, he would make his orifice near its lower surface. But that is not this case. In this case he wishes to retain the urine to keep the bladder distended, until he can hunt up and fish out with his forceps the stone or stones. If there are other objections they do not occur to us.

We are sensible that we shall at first shock the prejudices and previous associations of able and experienced lithotomists. We wish to be permitted to inform such, that what we have written is not a mere closet speculation —but that we have dissected, and seen dissected by others, the parts concerned in this operation repeatedly, and that we have performed the operation of opening the bladder on the dead subject, both by the posterior and by the anterior method, with a view of determining which ought to have the preference. The result of these comparative trials has been to us highly satisfactory. Indeed we cannot now perceive why this hitherto most difficult and dangerous operation is not rendered as safe for the male patient, (although it may take a little more time to perform it,) as for the female, which last operation is not thought much of by surgeons.

It may not be uninteresting to take into consideration some of the causes which may have contributed to prevent a discovery of this sort, especially when it is considered what a variety of plans have been contrived to get into the bladder. Surgeons have been in the habit of considering the introduction of the grooved staff and cutting into the bladder, as one operation. They should have been regarded as two. This has led to the mistake, since the position which is proper for one of these operations, can never be convenient for the other.

Another circumstance may be mentioned. The anatomist is in the habit of first dissecting the abdominal and other viscera, his subject very properly lying on the back. Pre-

vious to dissecting the perineum he introduces the staff, for doing which, he has no occasion to alter the position of his subject. To obviate the inconvenience arising from the anterior part of the pelvis projecting in his way, he places or puts under it some hard substance, in order to raise it up so as to enable him to get at his work. It never occurs to him, that by turning over his subject, all difficulties would vanish. Besides he is taught that it should be done in this manner with a view to the operation of lithotomy. Thus one error begets another and is propagated from age to age.

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Art. V. *An Essay on Uterine Hemorrhage.*

By W. P. Dewees, M. D.

THE frequency of occurrence, and the dangerous tendency of uterine Hemorrhage, render it peculiarly interesting to the medical practitioner. And this interest is increased when he considers the discordant opinions entertained of its origin, and but in too many instances the inefficacious modes proposed for its treatment. With a view to reconcile the first, and to point out for the second a more decided plan of cure, the present observations are offered.

In the earlier writers upon this subject, we find considerable diversity of sentiment, not only with respect to the source of the disease, but also as to the mode of treatment. While some were of opinion that the neck of the uterus and a portion of the vagina furnished the expended blood, wherever abortion or miscarriage did not take place ;* others contended, the discharge could only happen from a separated portion of the placenta or membranes.† A few were content to rely upon the efforts of nature, and thought it might be even mischievous to interrupt it—or at farthest

* Mauriceau, La Motte, De Graaf, &c.

† Puzos, Pasta, Kik, De Graaf.

depended upon temporising applications to the vagina, or upon the exhibition of some inadequate astringent by the mouth—while, a much greater number insisted it was a most serious accident, and that the woman's safety exclusively depended upon the expulsion of the ovum, or the premature delivery of the child—from which it would appear, how vague and uncertain their notions upon this subject were.

We are not, however, to include in this censure, the later writers upon midwifery. From them we confess to have received much important information, and to be indebted *collectively* for nearly all we know of its mode of treatment—though we must at the same time declare, we do not regard any *one* of them as having brought into view all that we consider valuable, or necessary to be known, upon this formidable complaint.

In prosecuting the inquiry into this subject, we do not feel ourselves bound to give a detailed account of the notions entertained by every writer within our reach, and shall merely pledge ourselves to the faithful selection of such opinions and observations as may in our estimation merit most consideration. In doing this, we hope we are performing an acceptable service to those whose leisure or opportunities will not permit them to do it for themselves; and sincerely trust we shall be forgiven when we are found to depart from high authority or generally received opinions, which do not comport with our own views or experience. For the latter, we can with much honesty declare it has been rather ample than confined.

The mode of inquiry we propose to pursue is—

First. To consider very briefly the nature of the connexion of the ovum with the internal face of the uterus.

Secondly. To investigate the causes which may impair this connexion, and thus expose the source from which the blood is derived.

Thirdly. To examine into the mode of action of these agents in effecting this lesion.

Fourthly. To point out the several periods of utero-gesta-

tion at which this may take place—and trace the various consequences which may result from these periods.

Fifthly. To notice the mode of treatment under the different stages, and circumstances, which may accompany the disease.

I. *The connexion of the Ovum with the Uterus.*

Soon after the ovum is deposited within the cavity of the uterus, we find it connected through its whole extent of surface, with the internal face of this organ. Both uterus and ovum contribute to this end—on the part of the womb, we find it produce a soft spongy substance called decidua—on the part of the ovum we discover its external covering or chorion shooting out innumerable vascular fibres—and both when united serve as the bond of union between ovum and uterus. The efflorescence on the uterine surface, like that which covers the ovum is decidedly vascular—and it would seem, that these minute vessels, interlock with each other after a certain period, so firmly, that they cannot be well separated without rupture.*

It is not necessary to our present purpose to inquire in what manner these vessels subserve the purposes of fœtal growth—we only clearly understand, that when the integrity of either set be injured, there will follow a discharge

* Mr. Burns, (Principles of Midwifery, p. 181. Ed. 2.) who is high authority, is of opinion, that a separation of the maternal and fœtal portions may take place—this may be, though not very susceptible of proof, nor is it perhaps of any great practical importance, yet if it be a fact, it should be recorded as such. He says, "at times the fœtal and maternal portions separate, and the first is expelled before the second, forming a very beautiful preparation." We have seen several ova of the kind we believe Mr. B. alludes to, but their appearance would lead us to a very different conclusion. But for fear we may misunderstand him, we will describe what we have seen more particularly. In several instances we have known ova expelled from the uterus, after a considerable continuance of pretty severe efforts, which were decidedly without a vestige of the uterine product attached to them—they were evidently, and indisputably, examples of the ovum being surrounded or covered by the entire product of the chorion, without their having formed any union with the decidua—their discharge was not attended by hemorrhage, though there was slight discharges of blood. We own a very beautiful preparation of this kind at this moment.

of blood, proportionate to the extent of injury—the part of the uterus at which it may happen, and the advancement of gestation. Should a large portion of the ovum be detached in the earlier months, the quantity of blood that may issue will be commensurate with that surface, especially if it be from the body or fundus. If the separation take place near the neck, the discharge will not perhaps be so abundant, as this part is considered to be less vascular than the other portions of this viscus.

But the latter of these circumstances will be influenced by the period of gestation. As a general rule it may be said, that the quantity of blood which may be expended will be in proportion to the advancement of pregnancy.

II. *The causes which may tend to destroy this connexion.*

In consulting authors upon this subject, we shall find that a variety of causes are enumerated as capable of destroying, to a greater or less extent, the connexion between the placenta and uterus—and it is agreed by far the greater number, that no considerable hemorrhage can occur unless this happen. By a few it was believed that a mere separation of the membranes was sufficient for this purpose: but of this we have no good evidence—and should it be even true, it can only refer to this accident after the fifth month—for until this time, the whole uterus may be considered as being lined with placenta. Mauriceau, Lamotte, and others, supposed that a discharge of blood during pregnancy must necessarily produce abortion, if it proceeded from the proper cavity of the uterus, as this could only happen by a destruction of continuity between the ovum and uterus: while Dionis thought this could take place without such a consequence, as he imagined the placenta occupied the fundus of the uterus alone, and that those vessels which furnish the menstrual discharge before impregnation, might also yield it during gestation, if they were in a plethoric state. De Graaf was of nearly the same opinion. It would be a waste of time to labour this point farther at this place. We shall only say, that we agree with those who

derive the blood expended in flooding from the exposed surface of the uterus, by a portion or the whole of the placenta separating from it.

In enumerating the remote causes of hemorrhage, we shall only name such as are most generally believed capable of this effect. 1st. A too short Funis: 2d. Mechanical violences: 3d. Passions or emotions of the mind: 4th. Plethora: and after delivery. 1st. Atony: 2d. Spasm: 3d. Humoral engorgement: 4th. Unequal contraction of the uterus: 5th. Inversion.

Though all these causes have been assigned for the disease we are considering—still it is sufficiently difficult of explanation how some of them act to produce it. When violence of any kind is offered a pregnant woman and she miscarry, or is prematurely delivered, the cause, from its force or extent at first sight, appears capable of the end, and there all investigation ceases. It may not therefore be time ill spent, to inquire into their respective agencies.

III. *Mode of action of certain of the remote causes.*

And first of too short a cord. It was the opinion of Lamotte, that the cord may be naturally or accidentally too short—in which case it might be the cause of hemorrhage. He gives a case illustrative of this assumption—but confesses it was the first and only one he ever met with. The bleeding proceeded from one of the umbilical vessels, at a portion which was folded into a kind of knot, which part yielded from the accidental shortness of the funis. Levret met with a similar instance.* It must however be confessed by all conversant with the practice of midwifery, that though this may be a cause of hemorrhage, it must be a very rare one—or the extensive practice of these three celebrated authors would have furnished more examples.

It is not at all extraordinary that we should have only a few cases of this kind upon record, since we do not well perceive how it can take place. Though the cord may be very short, either naturally or accidentally, still there must

* Baudelocque also mentions a remarkable case of the kind. Midwifery, par. 1084.

be great difficulty in breaking it by any effort of the child, for if the waters be preserved, the specific gravity of the child and them will so nearly correspond that the weight of the child may be considered as almost nothing—so that whenever the cord was put upon the stretch, the child would instantly move towards the force, and thus destroy its influence. If on the other hand, the uterus should be emptied of the waters, it would instantly almost embrace the body of the child so firmly by virtue of its tonic contraction, as to render it almost immoveable, and consequently it could not exert so much force as to injure the continuity of the cord. We may then safely conclude, that if it take place, it must be attended by such a combination of circumstances, as will always render it of extremely rare occurrence.

Another inconvenience is said to arise from too short a cord—namely, a separation of a portion or even the whole of the placenta during labour. Leroux says "that the placenta may be separated entirely or in part in consequence of too short a cord. This case," he says, "is met with in practice, and he is persuaded that the greater part of the floodings which happen during labour after the escape of the waters, and when the head is in the lower strait and the pains are almost useless, has no other cause."* We were no little surprised at this declaration, as we did not recollect a single instance, nor could we find it among our notes, where the hemorrhage was attributed to this cause. And we are firmly of opinion, that whenever too great a shortness of cord shall become a cause of flooding before delivery, there must exist at the same time, a preternatural feebleness of union between the placenta and uterus: for if the usual degree of adhesion obtain, the cord would break, before the placenta would separate, as the force which it would exert upon this mass would be at right angles with its surface, and would require a much greater power to separate it, than could possibly be employed by any movement of the child, and more especially at the time indicamely, after the discharge of the waters.

* Pertes de sang, par. 162.

Besides, were it possible that this cause should produce a separation, it would most probably be opposite to the point of insertion of the cord, which generally speaking, is near the centre of the placenta: if it effected it here, it would almost certainly be concealed, as the surrounding attachment would act as a dyke to the influent blood, and thus conceal the injury at least until after the birth of the child. But, Leroux himself confesses the discharge is not great, and is more dangerous to the child than to the mother—and also that he has received many children which did well, notwithstanding the cord did not exceed in length six inches. Many authors mention this uncommon shortness of cord, and mention it in such a manner as to lead to the conclusion that it is more frequent than is imagined. It may be so—though we have never in a single instance met with a funis, the natural length of which did not nearly double this.

We grant a too short cord may be extremely inconvenient, and create considerable embarrassment at times, especially after the head is protruded through the external parts: at this time all the accidents stated above may happen, and can only happen then.

2dly. Mechanical violence: 3dly. Passions or emotions of the mind: 4thly. Plethora. Each of these causes may produce uterine hemorrhage, and they all perhaps have in their turn done so. However, the mode in which they effect this, is not so well understood as it may deserve—the whole of these causes have one operation in common upon the system—they all induce an increased force of circulation, and which is generally considered sufficient under certain circumstances to produce the evil in question. It has been thought, that whatever gave an increase of force or velocity to the circulatory system of the mother, must almost necessarily, in consequence of the large size of the hypogastric and spermatic arteries—the short distance they have to travel before they arrive at the uterus, together with their great increase in that viscus as gestation advances, very much affect the condition of the ovum within its cavity—that the arterial vis a tergo must act mechani-

cally upon the ovum, and by mere force of circulation drive it from its connexion with the uterus—that plethora must act pretty much after the same manner—and as a proof of this, it is said that the periods at which the menses are wont to return, are those at which abortion is most readily provoked: for at these times, though the uterus is impregnated, and this discharge has ceased, still the blood is sent in greater abundance than usual, until the demands of the embryo are such as to employ it, without suffering the vessels to become engorged.

But those who reasoned in this manner did not seem to have a very clear idea of the nature of the union between the ovum and the uterus, since they differed as to the mode. While some insisted that the blood was transmitted *pleno rivo* by continuation of canal from the mother to the placenta, others did not think this necessary, as mere turgescency within that mass was all sufficient for the end proposed. Though we do not mean to deny altogether the influence of an increased circulation—we are disposed to very much limit its agency in producing a separation of the ovum, either in part or entirely from the uterus. For, were a mere increase of circulation all that is required to effect this end, no woman should escape aborting who may labour under high arterial action—thus fevers of all kinds should be followed by this accident, which is contrary to all experience. We are obliged then to suppose something more necessary than an vigorated circulation, to produce this effect.

We might indeed insist, that nature has attempted with some success to guard against this contingency, by the peculiar construction of the uterus itself. In the early months there is comparatively but a small quantity of blood sent to the uterus, because the necessity for it is comparatively small—and the force of even this is diminished, by its passing through vessels of small size, and much folded, or convoluted.* This provision is highly important to the welfare of the ovum at this period, since its connexion is

* Baudelocque, &c.

not so well established as it afterwards becomes, as gestation progresses. The liability therefore to abortion is greater in the early, than in the later stages of pregnancy—for as the union between the chorion and decidua is not yet well confirmed—as the attachment of the latter to the internal face of the uterus is proportionably slight—and as the extent of surface which the ovum now presents is very small to that which it offers in the more advanced states of pregnancy, and as it can of course be affected by smaller causes, it will be seen that a separation will be more easily induced, and prove much more injurious to the well being of the embryo, than a larger one at another stage.

In the more advanced periods of utero-gestation, the circulation becomes freer, and the vessels pretty rapidly increase in size.* Yet, as we have just intimated, the woman is not so liable to the accident we are considering—now, were nothing more required to induce hemorrhage than an increase of circulation, why should it not more readily occur at this time, than earlier? since it must be admitted, that more blood is now sent, because more is required—that the vessels are much larger—and arterial action increased in the exact ratio of their augmentation. To comprehend this, we must advert to another part of the uterine economy, in which nature appears to have been studious of the safety of the ovum, by a new provision in organization. Thus however much the vessels of the uterus may have augmented in size, those which directly administer to the necessities of the fœtus, do not alter in the same proportion. There is every reason to believe that the relative sizes of these two sets of vessels bear a much greater relation to each other in the early, than in the later months—so that the risk of injury from an impetuous circulation is diminished instead of being increased.

It must however be understood, that a given space of exposed uterine surface, will yield blood (cæteris paribus) in proportion to the advancement of gestation, because the vessels which furnish it have increased, in proportion to

* Baudelocque, &c.

this advancement. Now should the deciduous portion of this viscus be removed, it would necessarily expose the extremities of those vessels which yield a supply to an infinity of others, which terminate in, and in part constitute the placenta.

We know of no one who has clearly explained the manner in which the blood is conveyed into the minute vessels which constitute the decidua. That there is however, a peculiar arrangement for this purpose is certain, because there is an absolute necessity for it, since, were the blood conveyed to the ovum *pleno rivo* by vessels of the same size as those which furnish it from the proper substance of the uterus, or even of much smaller capacity, but subject to the same impulse, it follows, that it would be liable to injury from every increase of arterial action, which, as we have attempted to prove, is not the case.

Besides, injections prove that a portion of the decidua can be completely filled—and that it consists of infinite congeries of vessels, whose respective size bears no proportion to those terminating immediately upon the internal face of the uterus, or those which are directly interested in conveying blood to the ovum.

Is it not more than probable then, that each vessel which may terminate in the uterine cavity, has a great number of very fine ones corresponding with it, and which constitute in part the decidua? Is this not the mode which nature has adopted, to prevent the evils which must necessarily result from a hurried circulation? Is this not partly proved by the fact, that when the placenta is removed, and the uterus does not contract, that we have an overwhelming flooding? and may we not add, that such a contrivance is essentially necessary to the well being of the ovum, as well as to the security of the woman after she has expelled it? for were it otherwise, we should always have a rupture of vessels upon the separation of the ovum, or upon the casting off of the placenta from the uterus—but agreeably to this scheme, we have only an exposure of their extremities, which the contracting uterus almost immediately shuts up.

The decidua then performs two most important offices in the economy of gestation: first, by its great vascularity, as we have just pointed out; and secondly, by its sponginess and compressibility, which arise from the disposition of its vessels. We trust we advance no absurdity when we say, that most probably one of the uses of the decidua being so cellular and compressible in the early months of pregnancy, is to obviate consequences which might result to the feebly fixed ovum, were it otherwise, from external violence, or internal impulse. By its interposition and softness, vibration, however excited, would in part be certainly arrested in its progress to the ovum—and in more advanced gestation, the same immunity from risk of this kind would follow, from the peculiarly soft and yielding texture of the placenta—for at this time, injury could only happen from a separation of one of its portions—a disunion of the membranes yielding little or no blood.

From what has been said, we think we have rendered it probable, that something more is required than an increased force of circulation to effect a separation of the ovum in the early months—or of the placenta in the more advanced periods of pregnancy—and that something we believe to be uterine contraction—as without this we are at a loss to understand the *modus agendi* of the remote causes. We shall not pretend to say how the various causes we have enumerated above induce this action—though we are certain that this effect is produced through their agency, and for the following reasons. 1st. Because, mere circulatory impulse appears from the anatomy of the uterus and ovum to be inadequate to this effect—since neither abortion nor premature delivery follows as a consequence when this condition has been present in its highest degree. 2dly. Because, contraction in every instance is essential to the separation of the placenta, in abortions, premature labour, or delivery at full time. 3dly. Because, we frequently detect this cause, hours, or sometimes even days before the eruption of blood; and because, so long as this contraction continues, hemorrhage will not cease, unless we diminish the

bulk of the ovum, or interrupt its return by proper remedies. We are aware that objections may be raised to the reasons just given: it may be said that all testimony is against our first, as we are told by writers from the time of Hippocrates downward, that plethora is frequently a cause of hemorrhage, and that abortion is often prevented by the loss of a few ounces of blood. Be it so. We also believe such to be the fact. But this is no contradiction, since this condition of the system may act very differently in separating the ovum, than by mere impulse. The vessels in the proper substance of the uterus will and must partake of the general fulness of the system. They are of course distended more than ordinary—in consequence of which they must act as so many wedges to the uterine fibres which, by being thus stimulated, are made to contract.*

To the second, it may be said, that we have no evidence of this in the cases under consideration. It is true, we have no positive evidence, but we have strong presumption that it is so. Thus, in those instances which fall immediately almost under our inspection, we find that the placentary mass is separated only by contraction—for when this does not take place, the after birth retains its adhesion with the uterus—hence, it is always solicited for this very purpose when absent at the termination of labour.

To our third, it may be objected, that in many instances hemorrhage comes on without being preceded or accompanied by the slightest pain. This though we also admit, does not prove there has been no contraction of the uterus, for we well know that pain is not essential to this end—The uterus may and does contract, and sometimes with great violence, without the addition of pain—and which is

* We cannot perhaps better illustrate our idea of the connexion of the vessels of the decidua with those of the uterus, than by comparing them to fine camel's hair pencils—the quill part to represent the uterine vessel, and the hairy fibres the vessels of the decidua attached to it—the calibre of the quill being equal to the area of the hairy fibres—by this arrangement, the circulating force will be necessarily so much diminished by its almost infinite division, that little injury can be sustained by the ovum, by a mere increase of circulation.

well illustrated, by what almost always happens after delivery, namely, the spontaneous separation of the placenta as it is termed, in which case, contraction is unaccompanied by pain—and also, by what very uniformly takes place previous to the painful state of labour, the alternate contractions of the uterus, as detected by passing a finger into the os uteri, where it will be found, that the membranes are alternately tense and relaxed, and if a hand be applied to the abdomen, the uterine globe will be felt to harden and relax, as contraction may be present or absent. Yet during this time no pain is experienced. Furthermore, by that peculiar contraction of the uterus called the hour-glass contraction —where the placenta is imprisoned in the upper chamber of the uterus by the body contracting very forcibly below—and so firmly does it maintain this condition, that it requires no common force to overcome it—yet there is no pain.

To the fourth it may be observed, that pain, when it attends, is rather a consequence than a cause—for such disturbance has been given to the uterine economy by an increased circulation, as to call in the aid of pain to free itself from the useless burthen, as the ovum has now become, because of its extensive or entire separation from the uterus, and must be considered rather as an extraneous body than a living substance. To this we would answer, that it is sometimes strictly true as regards the ovum, and is an event which always takes place when the embryo or fœtus has lost its life. But does it follow, that because pain, which must be considered only as an evidence of contraction, becomes necessary for the expulsion of the ovum, that it may not have existed before, or that contractions may not have been often repeated without manifesting themselves by pain? certainly not. Besides, we know that painful contractions may have accompanied hemorrhage for a considerable length of time without the ovum being destroyed, and the woman notwithstanding go her full time. Of this we, as well as others, have seen more than one instance. Yet had these contractions been permitted to have continued, they would inevitably have caused abortion.

Again it may be said, and truly, that uterine contraction may take place, and even of a very powerful kind, without being followed by a separation of the ovum or placenta—Were this not the case, indeed, hemorrhagies and abortions would be much more frequent than they are. But to obviate the force of this objection we may only state the fact, that ova do not always adhere with equal firmness to the uterus, nor does even the placenta itself, after it has become located to a certain portion of the uterus. Of this we have abundant proof in the many instances of habitual aborting, and in those where apparently very slight causes will occasion premature labour or early miscarriage. Some women appear to have ova so ill established, as to require but the slightest mental emotion or corporal exertion to have them cast off. This is notorious to every practitioner.

It may also be alleged, that those cases of hemorrhage which are accompanied by pain, consequently by contraction, are less dangerous, and of more easy management, than where this does not obtain. Now were contraction necessary to produce this disease, how is it that it can serve to remove it? This presents no difficulty. The whole truth is not told. Where the ovum is about to be cast off either in the early, or later periods of the pregnancy, or where there is no chance of its preservation from the effect already produced upon it, then contraction is useful, as it proves the healthy disposition of the uterus, so far as this circumstance is concerned. By it, the ovum is completely separated—and cast off,—the bleeding put a stop to, and the woman secured from danger. But, let us ask any practitioner of experience, whether he has not uniformly found those cases which have been attended with pain, always of more difficult management, than where none existed? We are sure he will answer yes. Now, if this be true, and that it is so cannot be doubted, does it not decidedly prove that contraction tends to increase the disunion or maintain the separation, as well as to have produced the original lesion? This fact too is so notorious, that every body who has a view to the security of

the ovum, endeavours in the first instance to diminish or destroy uterine contraction by the exhibition of such remedies as may be capable of such effect.

It may not be amiss to inquire, how far we may have a control, or whether we have any, over uterine contraction after it has once been called into action. The no small authority of Mr. Burns is against us when we say, we think we have—though confessedly difficult of subjection. Yet, as it is a matter of high consequence to ascertain the truth upon this subject, we hope to be forgiven if we differ from that respectable writer. He says, "when abortion is threatened, the process is very apt to go on to completion, and it is only by interposing, *before the expulsive efforts are begun*, that we can be successful in preventing it; for whenever the muscular contraction is *universally established, marked by regular pains, and attempts to distend the cervix and os uteri*, nothing I believe can check the process."

That it is a matter of uncertainty whether we succeed in our attempts to arrest uterine contraction after it is "established" must be acknowledged. But that it is never attended by success we cannot concede—nor should the principle ever be inculcated, as it paralyzes exertion, and withholds from the suffering female that comfort which the attempt rarely fails to give. Our own experience would, we think, in more instances than one declare, that we have been rewarded for the attempt to interrupt uterine contraction—and should it fail nineteen times out of twenty, we are surely not justified in withholding the probable means. We therefore make it an invariable rule to treat the case as if we expected to meet with success—and have had reason to suspect we are not doing our duty. There is one case however, in which we never interfere with the slightest prospect of an happy issue—and that is where the process of gestation has unequivocally ceased—and of which we take but one circumstance as absolutely certain, namely, where the breasts have become tender and tumid, and then pretty suddenly

subside. It would here be a forlorn hope to administer remedies with a view of retaining the ovum.

We are disposed to believe that this circumstance is the only one which marks the loss of life of the ovum with sufficient certainty: it is perhaps the only one that is unequivocal, since all others may be said to be deceptive. This mark was known to Hippocrates, and has, we believe, ever since his time stood the test of experience. So long then as this sign be absent, we do not relax in our attempts to preserve the ovum. It must however be confessed that we have known the ovum cast off where this symptom was wanting. Yet we are persuaded in each of these instances that the ovum preserved its vitality almost to the last moment, and that its expulsion was owing to the indomitable nature of the contractions of the uterus—and we think that this has obtained most generally with women who are in the habit of miscarrying. We do not stand alone in our opinion upon this subject. Puzos (Mem. de l'Acad. de Chirur. Vol. I. p. 203.) declares that neither pain nor hemorrhage necessarily produces abortion. Lamotte (obs. 305.) gives an instance where the woman went her full time after the orifice of the uterus was considerably dilated. And above all we may cite Mr. Burns, himself, for an example most strictly in point. (Princip. of Mid. ed. 2d. p. 195, in a note.) He relates with seeming belief that cases have occurred of twins, one of which has been expelled, while the other remained, and the "action of gestation," as he happily terms it, was still maintained to the proper period. Now this is demonstration that after muscular action has been universally established, it can be suspended for a considerable time: if this be so under the circumstance of one fœtus being expelled, and the uterus by a cessation of action shall permit a second to remain until the proper time, we *à fortiori* should expect it when the uterus is not so extensively or powerfully excited. Besides, we might urge cases related by both Mauriceau and Lamotte, where the uterus was emptied of its waters, and yet the women went their full time, though they were not within six or seven weeks

of it—In these instances the uterus could not fail to have contracted. We however must fully agree with Mr. Burns, that where the "action of gestation" has ceased, it would be unavailing if not injurious to attempt the preservation of the ovum—for it *must* sooner or later be cast off. Denman is also of opinion that uterine contraction can be subdued. He says, "that experience has fully shown, that women who have had not one, but repeated discharges, with *considerable and regular pains*, have gone to their full time." Introd. to Mid. p. 472. Francis' Edit.

The remote causes which we have hitherto been tracing, may with much propriety be considered as contingent or accidental in their application and influence. But there is one still remains to be noticed, which must be regarded as absolute in its effects, whenever it may chance to exist—we allude to the implantation of the placenta over the mouth of the uterus.

The knowledge of this particular location of the placenta is of modern discovery—and perhaps Levret is the first, who decidedly taught this doctrine. Mauriceau, Lamotte, and others before his time, met with the placenta in this situation, but they all believed it was a mere precipitation of this mass, after an entire separation from the fundus of the uterus. The whole process of generation is involved in such complete obscurity, that conjecture is constantly made to supply the place of facts, or of well ascertained processes. It would seem that the daring, or hardihood of the theorist was augmented in proportion to the obscurity of the subject, or the difficulty of ascertaining truth—hence we have nothing to rely upon but conjecture, on the manner in which the placenta becomes situated over the os uteri—nor shall we perhaps ever be more enlightened than at present upon this subject. Generation with all its attendants most probably will ever remain among the inscrutable arcana of nature.

Lereux,* says "Lorsque l'œuf humain fécondé a par-

* Obs. sur les pertes de sang. p. 13.

coura le trajet de la trompe, et est tombé dans la matrice, il se trouve dans une cavité qui est beaucoup plus ample que le canal d'où il sort. Son pédicule, qui doit former le placenta, *et qui est sorti le dernier de la trompe*, reste le plus ordinairement supérieur; cependant, comme l'œuf est encore flottant, le pedicule peut se tourner par quelque accident plus on moins inférieurement."

Mr. John Burns* follows Lereux very *closely* in his conjectures, or rather his assumption of facts upon this subject. He says, "as that part of the membranes of the ovum to which the embryo is attached, generally enters last, it follows, that the placenta will be formed originally over that part of the uterus where the tube enters the decidua, at that spot joining with the chorion to form it. But in some instances the case is reversed, and the embryo enters foremost, the rest of the membranes following it. When this happens, then the inner layer of the decidua which was stretched across the orifice of the tube, and which is afterwards to become the decidua reflexa, will contribute to the formation of the placenta. In this case, by the distention of the ovum, and the yielding of the decidua reflexa, the placenta will come at last to be inserted over the mouth or over some inferior part of the uterus."

In this manner do these writers account for this unnatural situation of the after birth—The only difference in their views is, that Lereux, not understanding the nature of the decidua, or perhaps ignorant of it existence,† supposed that the ovum, after it was deposited in the uterus, was unconfined, or rather floating in its cavity, and might in consequence of this, by some accident turn its "pedicle"

* Gravid uterus, p. 153.

† It is remarkable (so far as we at present recollect) that none of the French physiologists have faith in Hunter's description of the decidua. Baudelocque, Meygrier, and Gardien, declare, if it exist at all, it is only in the early months of gestation, and then perhaps only observable towards the lower part of the uterus. From our own observations, we have no hesitation to declare its existence, but not precisely as laid down by either Hunter or Burns.

which was to become placenta, downwards, though it generally remained upwards, and thus become situated over the os uteri, while Mr. Burns supposes the portion which is to constitute this organ, enters the uterus by some chance first, and thus will have or assume this inferior situation.

From this it will be seen, that much is taken for granted, which, as it can never be proved, one conjecture may be as good as another, provided it is not found at variance with any well established fact. In this instance perhaps hypothesis can do as little mischief, as in any case with which we are acquainted—and as all practical ends are answered by the knowledge that the placenta is sometimes thus engrafted, we shall not attempt a refutation of it, especially as we have none better to substitute.

The order of developement of the uterus is so uniform, that a deviation from it can only result from accident, or such a combination of circumstances as can very rarely happen. We can then with absolute certainty declare, that when the placenta is unhappily situated over the mouth of the uterus, a flooding towards the latter periods of gestation must be inevitable—hence, the propriety of the term "unavoidable," for this kind of hemorrhage.

During the first six months of utero gestation, the body and fundus alone yield to the distending power of the ovum: after this time the neck is called upon (if we may so term it) for its proportion, as the other parts of this organ seem to refuse any further supply—in consequence of which, it, in its turn, becomes distended, and in this act, a portion of the placenta is necessarily removed—and a bleeding, according to the extent of injury, or the number of vessels exposed or ruptured, ensues. After discharging more or less blood the hemorrhage may cease, or be so reduced in quantity, as to excite little apprehension. But this is a false security—it is sooner or later renewed, either by a farther stretching of the neck, by the augmentation of the ovum, or by the removal of the coagulum which had until now stopped the bleeding.

In this manner may things proceed until near the last

stage of pregnancy—or the extent of separation may be such, or the size of the vessels exposed be so large, that the woman's life is instantly jeoparded, and from which she can only be protected by the most prompt, and decided remedies.

IV. *The periods of pregnancy at which hemorrhage may take place.*

There is no period at which this may not take place, after the first month of pregnancy, since it is presumable, that after the fourth or fifth week, a union more or less strict is formed betwixt the ovum and the uterus by means of the chorion and decidua: it must therefore necessarily follow, that a separation may be effected, and a bleeding ensue. Until about the fourth, or between it and the fifth month of gestation, this accident may happen to any portion of the ovum, since up to this period, the placenta or what is to become placenta completely surrounds the ovum.*

After this time, there is a portion of its surface that becomes transparent, and which uniformly augments in extent, so long as the uterus continues to increase in capacity. This transparent portion is what is technically called the membranes—and towards the full completion of pregnancy they occupy a larger surface than the placenta, from which they appear to emanate. In consequence of this, there is a portion of the uterus from which no hemorrhage can proceed, so soon as this transparent portion shows itself—and this portion relatively increases as gestation progresses—

* We believe that the whole of the vascular covering, until the time above indicated arrives, is destined for, and converted into placenta—We do not believe any of these vessels become "blighted," (Burns' gravid uterus, p. 196.) as it would seem to be a work of supererogation—and we believe further, that there is a point in every ovum from which the transparent portion of the membranes proceeds, and that that point is always opposite to the insertion of the cord into the placentary mass—this however is not the place for us to give our reasons for this belief; or we think we could satisfy on this head.

and of course, the source of flooding is confined to that part which is covered by the placenta—for all the remaining surface is lined by these membranes, and is incapable of furnishing such a quantity of blood as shall be denominated a flooding. We are aware that some have supposed the contrary of this, and declare that a separation of the membrane will yield sufficient blood for this purpose. But this is decidedly an error, since the whole transparent portion of the ovum is connected with the internal face of the uterus, by a very fine cellular substance only, in which very few red vessels enter, and these of very small size. Were this not so, we should have with every labour, a discharge of blood when the membranes protrude beyond the opened circle of the mouth of the uterus—which it is well known does not happen.

As a general rule then, we find the risk from floodings in proportion to the advancement in pregnancy—because the vessels are larger, and will in a given time yield a much greater quantity of blood—though the chance of occurrence is in the earliest months. Puzos says that abortions under the fourth month are rarely fatal—and this observation is perhaps confirmed by the experience of almost every practitioner—provided a sufficiently early attention has been paid to it. It must however be confessed, that it is very difficult to establish any certain rule upon this subject—since we have seen as alarming symptoms attend an abortion at six weeks, as we have witnessed from a premature labour of the seventh month, or indeed at any other period. It may however with confidence be advanced, that alarming symptoms do not show themselves as quickly in the early as in the latter months, and of course we have much more time for the employment of proper remedies. We may farther observe that it is frequently from neglect that any danger arises in the early stages of pregnancy—this inattention may proceed from the aversion that many women feel to let any thing be known that has any reference to their situation—from an ignorance of consequences, and from a long established opinion that a moderate discharge

is useful, especially in plethoric women, &c. Time, of great consequence, is lost by this improper procrastination, and many an ovum has been cast off, attended with threatening hemorrhage, which by early attention and proper care might have been preserved. Besides, the period of gestation has sometimes been permitted to lull the practitioner into dangerous security. Many of considerable experience maintain, that they have never seen danger from floodings before or at the period of three months. This is decidedly an error, and the sooner it is corrected the better. Whenever there is pregnancy with flooding there is danger—nor will the period of advancement, however short, protect of itself, against hazard. Of this, Mauriceau, Lamotte, Gifford, &c. give us examples—and we may add, our own experience furnishes the same results.

We are not to wait for extreme symptoms before we act: it is this delay, which creates in most instances the danger—and sometimes it has its victim. The authors just mentioned, and more could easily be cited, have furnished us with cases, not only of great danger, but of death, before and at the fifth month. It is wrong then to treat such cases with indifference,*—for though death may not be the consequence—extreme weakness, or a state of subsequent ill health, or the calling into action of some latent disease, may result from it. In another point of view, it is highly important that early attention be paid to such cases—namely, the prevention of its recurrence: for after a woman has once aborted, there is no security against the second, and presently a habit of it is established, which the best devised means within our knowledge is not always sufficient to destroy.

(To be continued.)

* Rigby treats this subject with great indifference; and the weight of his authority no doubt has tended to perpetuate if not to establish a most erroneous practice in the early months of gestation—he says, "the treatment of floodings that come on before the uterus has acquired any considerable size, must be very obvious, and the consequences of them at that early period of pregnancy are seldom to be dreaded, as, if the patient lose blood from the arm, be kept cool, and in a horizontal posture, and such

ART. VI. *Remarks on the Fitness of Savannah as a Place of Residence for Consumptive Patients, communicated in a letter to the Editor.* By WILLIAM C. DANIEL, M. D.

I HAVE for some time designed addressing you upon the subject of sending persons with pulmonic affections to pass the winter months in the South. My observations have satisfied me, that the instructions usually given are not the most judicious nor best calculated to realize the views of the physician or fulfil the hopes of the invalid.

The doctrines I believe generally taught in the schools of the North, to be—that while a southern climate is to be sought, patients are particularly directed to avoid the *brackish air* of Savannah, and such situations as Augusta and Columbia, are recommended in preference. This, previous to my residence here, was my own opinion—but all that I have seen has convinced me that I was mistaken, and I am now fully satisfied that this place has decided advantages for consumptive patients over any other in the Southern States, either more removed from the sea-board, or more immediately exposed to the sea air. It is alike exempted from the keen mountain air of the interior, and the sudden changes and dampness of the sea shore. The winds coming from the mountains are broken, and in a great degree divested of their bleak, chilling character by their passing over our fine barrens, which extend one hundred and fifty miles to the west, and northwest—and the space between this and the ocean, tempers very much, and deprives of their dampness those winds coming from the northeast and east, and southeast. Be the causes, however, what they may, I repeat that experience has fully

mild astringents and anodyne medicines be administered to her as have been found by experience, to restrain discharges of blood, they will very frequently stop entirely, and the woman go on to her full time; and if this should not be the case, but the hemorrhage should still increase, it will seldom increase to a degree to endanger the life of the mother." *Essay on Uterine Hemorrhage, 5th Ed. p.* 2.

proved that the atmosphere of Savannah is infinitely better calculated for consumptive patients than either that of Charleston, Columbia, or Augusta. It exercises a soothing, lulling influence upon them, which all who have breathed it acknowledge. I have very frequently been called to visit such patients from the North on their way to Augusta, or some other interior position, and I have seen the same persons return to Savannah with very little if any alteration of their symptoms, who upon remaining here for a few weeks have exhibited the most remarkable amelioration of condition. I have again seen others who had travelled to the South by land, returning home in the spring by this place, and who after an abode here of a few weeks, have professed to feel themselves much improved. It appears almost unnecessary that I should say I consider such relief usually as only temporary—temporary however as in most cases it may be, it is of great importance to the sufferers—and where we cannot relieve, it is most desirable to respite suffering. But I do believe that much more advantage, and frequently of a more permanent character would be derived if the visit to our climate were made at an earlier stage of the disease than it commonly is—and I am also persuaded that many lives might be saved if such patients could be prevailed upon to pass the summer and fall months with us as well as the winter. The fear, however, of an autumnal fever prevents this, which fever, if I may place faith in the opinion of many respectable medical men, and in my own limited experience, usually effects a cure by transferring (as we suppose) diseased action from the lungs to the liver. I have seen several cases in which an attack of bilious fever restored to health persons obviously in the first stage of consumption—and in such, the debilitated state of the system and the abstemiousness of their habits forbid a degree of fever sufficient to place their lives in serious jeopardy. I am, however, going beyond the limits I had prescribed to myself. Yet, permit me further to remark on this point, that I have known several persons in the first stages of consumption leave this place in a condition much

improved from that with which they came, who have died the succeeding summer in the Middle or Northern States. What would have been the influence of *our* summer upon them? It differs very materially from *yours*—and since it is capable of imparting so much robustness and vigor to invalids from our fevers, may it not produce an excitement in consumptive patients after a southern winter highly injurious to them? These however, are questions which extensive observation can alone answer.

The most remarkable influence which the air of this place appears to exercise, is in giving repose to the patient. I have known them reach here harassed almost to death, with restless, sleepless nights, and upon the night succeeding to their arrival, they have enjoyed refreshing slumbers—and from that time many of their most distressing symptoms have been astonishingly alleviated. This was particularly the case with a young gentleman of New York, who being considered upon the verge of a consumption came to pass his winter with us. A residence here that season, improved him much more than did the previous one passed upon the borders of the Mediterranean. He left us under favourable promises of a recovery, and to realize which, he was induced to make a voyage to India, from whence he returned the following February in a very emaciated condition. Such were his sufferings in New York, that he was induced to sail, almost immediately after his arrival from India, to this place. He informed me that his cough was exceedingly troublesome and harassing. He had considerable fever, with profuse night sweats which were very debilitating. He said, that for the six weeks previous to my seeing him he had not enjoyed more than two hours of repose in the twenty-four. On the night of his arrival he rested well, and his perspiration was inconsiderable compared with what it had been usually before—his cough had evidently abated, and he already felt much more comfortable. In a few days his appetite returned, his sleep, which was extended to the usual healthy term, was refreshing—his perspiration subsided, and his cough ceased to be

troublesome. His improvement advanced so rapidly, that he was very soon quite a different man—and in a few weeks after his arrival, he sailed for Charleston upon a visit of pleasure. The influence, however, of the air of that place was such as to occasion his speedy return to Savannah. On his arrival here, he informed me that he ceased to feel so well soon after his leaving us, and that in Charleston he experienced the same inability to sleep—the same profuse perspiration, and the same troublesome cough which had affected him previous to his coming from New York. The symptoms again subsided as suddenly as upon his previous visit—and during a residence here of some months his improvement was constant and rapid. He entered into society, and partook liberably of its pleasures and enjoyments without inconvenience. He sailed for New York, in the summer of 1820, since which time I have not heard of him. He left us under strong hopes that his disease was subdued, which, however, I did not feel myself authorized to sanction.

This is undoubtedly an extreme case, and it is not just to suppose that others would derive a similar benefit under similar circumstances, from a visit to this place. Yet I feel no hesitation in saying (and I do it upon the authority of many sufferers) that the atmosphere of Savannah is better calculated for consumptive patients and those disposed to pulmonic complaints than that of any position in the Southern States of which I have any knowledge.

I am, my dear Sir,
very faithfully
Your friend, &c.
W. C. DANIEL.

Savannah, May 22, 1822.

bbbbbbb beams of 50 pairs each.
ttttttt troughs on a platform counterpoised by a weight so that it may be elevated at pleasure by placing the foot on a treadle; the troughs being supplied with acid, into which the plates pppppppp may be immersed.

Grooved wood

Grooved wood

Zink plate

Copper Case

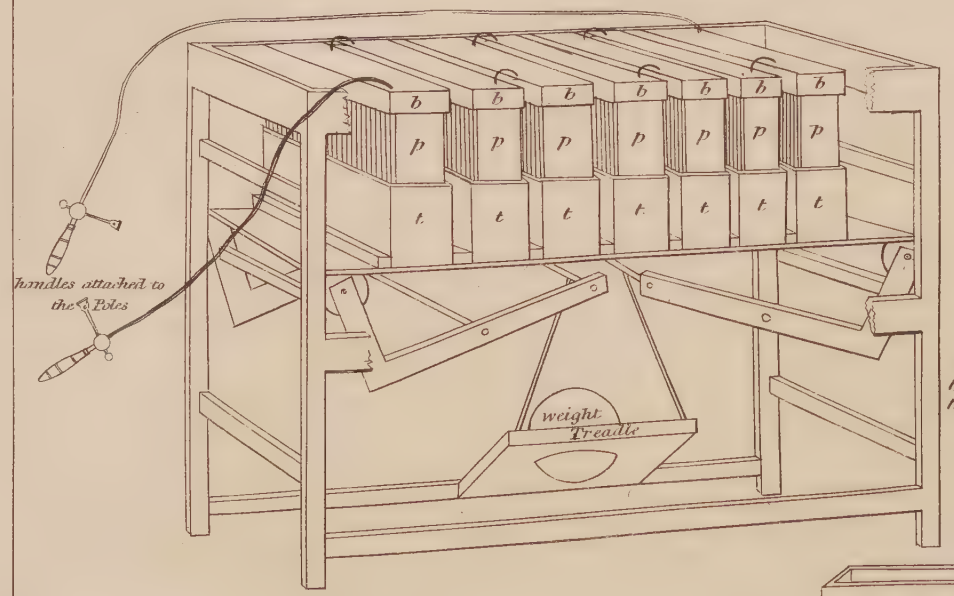
Ears to suspend and connect the series

Copper case

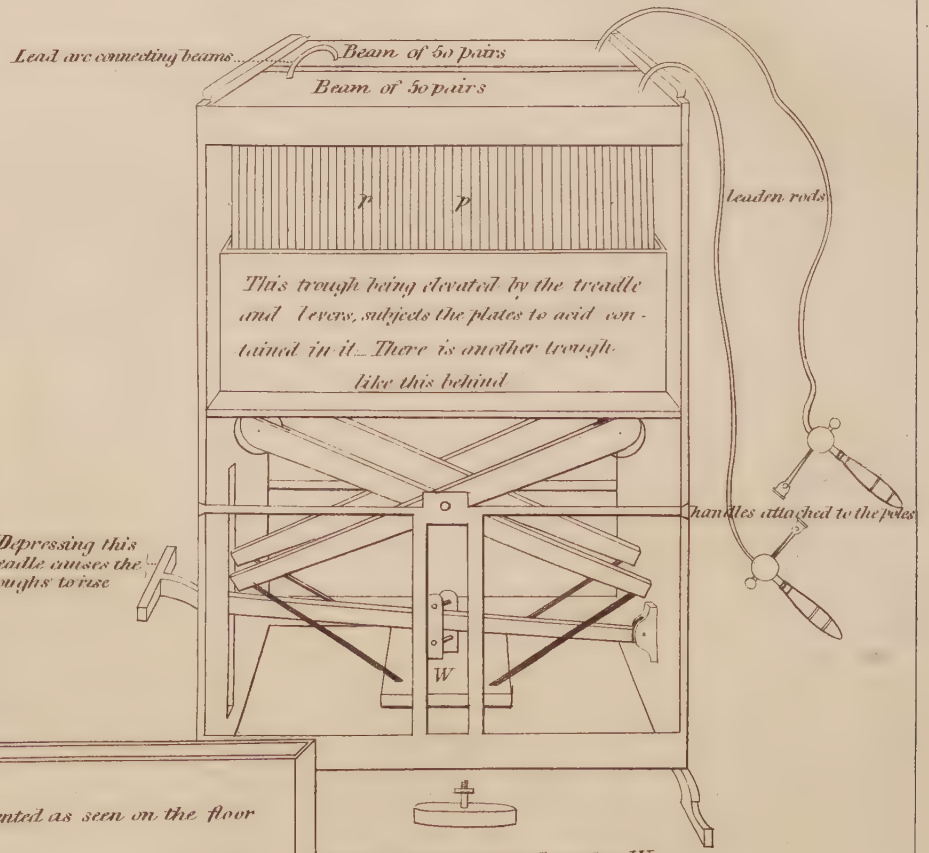
Zink plate

Between each case is placed a piece of pasteboard soaked in shell lac varnish

Deflagrator of 350 pairs



Deflagrator of 100 pairs


Lead arc connecting beams
Beam of 50 pairs
Beam of 50 pairs
p
leaden rods
This trough being elevated by the treadle and levers, subjects the plates to acid contained in it. There is another trough like this behind
handles attached to the poles
Depressing this treadle causes the troughs to rise
W

Trough represented as seen on the floor

Lead weight fastened under W.

ART. VII.—*Correspondence between* B. SILLIMAN, M. D. *Professor, &c. in Yale College, and* ROBERT HARE, M. D. *Professor of Chemistry, &c. in the Medical Department of the University of Pennsylvania, on the subject of* DR. HARE'S *Calorimotor and Deflagrator, and the phenomena produced by them.*

LETTER I.

My Dear Sir.

I WAS much impressed by your account of the Galvanic Deflagrator, and of the fine experiments which you performed with it, as described in the 3d Vol. (p. 105) of my Journal. By means of your kindness in sending me your original apparatus, (the only one which, as far as I am informed, has hitherto been constructed,) I had it in my power, early in the month of June, to repeat your experiments in my public course of lectures. Large numbers of intelligent persons attended, in addition to the classes, and the results gave great pleasure and satisfaction. My health being, at that time, very feeble, it was not in my power to pursue the subject to the extent which I had intended; and expecting to resume it, I had postponed the writing of a notice of your instrument, hoping that by and by, I could do it more to my own satisfaction. But as no one else appears to have repeated your experiments, I have concluded, even at this late moment, to throw a hasty notice into the Journal, although it has not been in my power to add any thing to the experiments performed in June.

I can say with truth that I consider your Deflagrator* as the finest present made to this department of knowledge, since the discovery of the Pile by Volta, and of the Trough by Cruickshanks. The vessels being filled with the fluid, beforehand, prevents any haste or confusion, and the advantage which your arrangement gives the operator, of immers-

* Your Calorimotor I have never possessed.

ing, at one quick movement, the whole of an extensive series, is very great. Being perfectly ready, and with the poles in his hand, the teacher only giving a signal to his assistant to immerse the coils, instantly directs the whole power to the desired point, and produces results, which both in brilliancy and energy, totally surpass any thing before effected by the same surface of metal, arranged in the same number of combinations. This will appear the more remarkable, when it is remembered that your apparatus produces these effects without insulation. Although through your civility I have just received the glass jars by which you insulate your coils, I have not yet been able to use them, and can therefore speak only of the results obtained without them.

With your eighty coils of fourteen inches by six, for the copper, and of nine by six for the zinc, I obtained effects which, as to every thing that related to intense heat and light, and brilliant combustion, far surpassed the powers of a battery of the common form of six hundred and twenty pairs of plates—one hundred and fifty pairs of which, of six inches square, are insulated by glass partitions—one hundred pairs of the same size, and three hundred of four inches square, are insulated by resin, and the rest, either by Wedgwood's ware or by resin, making in the whole a battery with a surface of thirty-six thousand eight hundred and eighty square inches. Your's has a surface of only twenty-two thousand and eighty square inches, *but even without insulation* it is incomparably more powerful than the other with that advantage. This is the most singular circumstance connected with your new apparatus, and which goes far to shake our previous theoretical opinions, if not to support your own.

I repeated every important experiment stated in your memoir, and with results so similar, that it is scarcely necessary to relate them. The combustion of the metals was brilliant beyond every thing which I had witnessed before, and the ignition of the charcoal points was so intense, as to equal the brilliancy of the sun; the light was perfectly

intolerable to eyes of only common strength. If I were to name any metallic substance which burned with more than common energy, it would be a common brass pin, which, when held in the forceps of one pole, and touched to the charcoal point on the other, was consumed with such energy, that it might be said literally to vanish in flame.

The light produced between the charcoal points when immersed beneath acids, oils, alcohol, ether, water, &c. was very intense, and platina melted in air as readily as wax in the blaze of a candle. It is a very great advantage of your Deflagrator that we can suspend the operation at any moment, with the same facility with which it was commenced. A look, directed to the assistant, is sufficient to raise the coils out of the fluid. All action instantly ceases—neither the metal nor the fluid are wasting any farther, and the lecturer is therefore at ease while he illustrates and reasons; and when he is ready, and not before, he proceeds to his next experiment. In the mean time, the instrument, during a certain period, rather gains than loses strength, by the raising of the coils. It seems as if the imponderable fluids, partially exhausted from it by its continued action, had time again to flow in from surrounding objects, and thus to impart new energy. I found the power of the instrument to last for several days, although declining, and the same charcoal points, when well prepared,* would also continue to operate for several days. When the coils, after immersion, had been suspended, for some hours, in the air, a coating of green oxid or carbonat of copper always formed on one part of the outside of the copper coils, and on the same part in all, but no where else. If I do not misremember, it collected next to the negative pole, but was, of course, always removed by the next immersion, though it was formed again at the next suspension.

One circumstance occurred during these experiments, which demands farther attention.

* By igniting pieces of mahogany beneath sand in a crucible.

In the hope of uniting the power of your Deflagrator, with that of the common galvanic battery, I connected your instrument with the powerful one mentioned above. Both instruments, *when separately used*, acted, *at the time*, with great energy, producing both their appropriate and common effects, in a very decided manner; but, on connecting by the proper poles, the battery of six hundred and twenty pairs, with the deflagrator of eighty coils, I was greatly surprised and disappointed, at finding the power of both instruments so completely paralyzed, that, at the points where a moment before, and when separate, a stream of light and heat, hardly to be endured by the eye, was poured forth—now, when connected, both instruments could scarcely produce the minutest spark. On separating the instruments, they both resumed their activity; on again connecting them, it was again destroyed, and so on, as often as the experiment was made. While they were in connexion, provided the coils were lifted out of the acid, so as to hang in the air merely, then the power of the common galvanic battery would pass through the Deflagrator, which appeared to act simply as a conductor, and as might have been expected, when so extensive a conductor was used, the power of the common battery was, in this case, considerably diminished, while that of the Deflagrator did not act at all.

If, while things were in this situation, the coils of the Deflagrator, without being plunged, were lowered so far as merely to dip their inferior extremities, say only one fourth of an inch in the acid, the communication was immediately arrested, and all effect destroyed almost as completely, as when the coils were wholly immersed. Thus it appears that the inability to act, in connexion with the common galvanic battery, depends upon the relation of the fluid and metal, and not upon that of the metals merely. These experiments should be repeated, with the aid of the insulating glasses, placed so as to receive the coils of your machine. I should be very curious to know whether the effects would be the same; and as I now have the glasses, I shall, as soon as possible, try this experiment. We must look to you, Sir,

for the explanation of this singular incompatibility between the two instruments. At present, I confess myself unable to explain it. It may, very possibly, lead to important results, and may have a bearing, such as I have not now time to discuss, on your own peculiar theory.

I would state that the mode of connecting the two batteries was varied in every form which occurred, not only to myself, but to several able scientific gentlemen, who were present at these experiments, and who were equally with myself surprised and confounded by their results.

I congratulate you upon the brilliant additions which you have made to our experimental means, in this department of knowledge; along with your invention of the compound blowpipe, they fairly entitle you to the gratitude of the scientific world, notwithstanding the uncandid attempts which, in relation to the blowpipe, I am sorry to see are still persevered in, to deprive you of the credit which you so richly deserve.

I remain, as ever, your friend and servant,

B. SILLIMAN.

Prof. ROBERT HARE, M. D.

LETTER II.

Philadelphia, November 5, 1821.

My Dear Sir,

I HAVE received your letter on the Deflagrator which I sent you last spring. I fear you have done me more than justice.

I should not be surprised, if the coils when insulated by the glass jars, should form a circuit with your other apparatus, better, than when immersed in the troughs. You will observe that when recently lifted from out of the acid, the air insulates the coils; while the pieces of wood used to keep the copper from touching the zinc, act to a certain extent like the moistened cloth in Volta's original pile.—When in this situation, the poles will affect an electrometer much more powerfully, than when the coils are immersed; though

in one case, the igniting power will burn a platina wire of one eighth of an inch in thickness, in the other it will not burn Dutch gold leaf.

In my memoir, on a new theory of galvanism, published in your Journal, is the following passage: "According to my view, caloric and electricity may be distinguished by the following characteristics. The former permeates all matter more or less, though with very different degrees of facility. It radiates through air with immeasurable celerity, and distributing itself through the interior of bodies, communicates a reciprocally repellent power, to atoms, but not to masses.* Electricity does not radiate in or through any matter, and while it pervades some bodies, as metals, with almost infinite velocity; by others it is so far from being conducted, that it can pass through them only by a fracture or perforation. Distributing itself of choice over surfaces only,† it causes reaction between masses, but not between the particles of the same mass. The disposition of the last mentioned principle (electricity) to get off by neighbouring conductors, and of the other (caloric) to combine with the adjoining matter or to escape by radiation, would prevent them from being collected at the positive pole, if not in combination with each other. Were it not for a modification of their properties consequent to some such union, they could not, in piles of thousands of pairs, be carried forwards through the open air and moisture, the one so well calculated to conduct away electricity, the other so favourable to the radiation of caloric."

Pursuing the same subject in a subsequent memoir, also published in your Journal, I thus expressed myself: "As yet no adequate reasons have been given why, in operating

* It cannot be pretended that electricity expands the gold leaves of an Electrometer when it renders them divergent, or that caloric causes any repulsion between the ignited masses which it expands.

† It is only when under a great restraint, that electricity enters the pores of metallic wires and deflagrates them. If it exist otherwise than on the surfaces of conductors, why does a hollow metallic sphere take as large a charge as a solid one.

with the pile, it is not necessary, as in the process of Van Marum and Wollaston, to enclose the wires in glass or sealing wax, in order to make the electricity emanate from a point within a conducting fluid. The absence of this necessity is accounted for, according to my hypothesis, by the indisposition which the electric fluid has to quit the caloric in union with it, and the almost absolute incapacity which caloric has to pass through fluids unless by circulation. I conceive that in galvanic combinations, electro-caloric may circulate through the fluids from the positive to the negative surface, and through the metal from the negative to the positive. In the one case caloric subdues the disposition which electricity has to diffuse itself through fluids, and carries it into circulation. In the other, as metals are excellent conductors of caloric, the prodigious power which electricity has to pervade them agreeably to any attraction which it may exercise operates almost without restraint. This is fully exemplified in my galvanic deflagrator, where eighty pairs are suspended in two recipients, forty successively in each, and yet decompose potash with the utmost rapidity, and produce an almost intolerable sensation when excited only by fresh river water. I have already observed that the reason why galvanic apparatus composed of pairs consisting each of one copper and one zinc plate, have not acted well without insulation, was because electro-caloric could retrocede in the negative, as well as advance in the positive direction."

Agreeably to these views, in order to prevent the escape of the electricity put into motion by the series, the caloric must bear a certain proportion to it. It is to be inferred, consistently with the same hypothesis, that this proportion did not exist in the series which you connected with the Deflagrator. The fluid presented to the latter had too much electricity in it; and hence, instead of passing into circulation, escaped. When the coils were suspended in air, this escape was less favoured than when they were covered by the diluted acid.

Faithfully Yours.

ROBERT HARE.

LETTER III.

Philadelphia, March 5, 1822.

My Dear Sir,

In reply to your inquiries on the subject of the Calorimotor, and the expediency of employing one during your lectures, it may be proper to mention, that the phenomena produced by it are more agreeable to the eye and therefore more popular, than any which can be performed without greater difficulty. By the time the Calorimotor is completely immerged in the acid solution, the wire in the forceps is rendered white hot, and takes fire, emitting the most brilliant sparks. In the interim, an explosion usually gives notice of the extrication of hydrogen in a quantity adequate to reach the burning wire. Immediately after the explosion, the hydrogen is reproduced with less intermixture of air, and rekindles, corruscating from among the forty interstices, and passing from one side of the machine to the other in opposite directions, and at various times, so that the combinations are innumerable. The flame assumes various hues, from the solution of more or less of the metals, and a blazing froth rolls over the sides of the recipient. When the calorimotor is withdrawn from the acid solution, the surface appears for many seconds like a sheet of flaming foam.

I refer you to the last paragraph of my memoir on the Deflagrator, for some results obtained by calorimotors, of different sizes, which I deem to be scientifically important.*

* The following is the paragraph alluded to:—

"The heat evolved by one galvanic pair has been found by the experiments which I instituted, to increase in quantity, but to diminish in intensity, as the size of the surface may be enlarged. A pair containing about fifty square feet of each metal, will not fuse platina, nor deflagrate iron, however small may be the wire employed; for the heat produced in metallic wires is not improved by a reduction in their size beyond a certain point. Yet the metals abovementioned, are easily fused or deflagrated by small pairs, which would have no perceptible influence on masses that might be sensibly ignited by larger pairs. These characteristics were fully

With respect to the comparative powers of concentric coils, of copper and zinc and of plates of those metals alternating; if only a few pairs are to be employed, I believe it a matter of indifference which construction we adopt. I have however, found to my cost that it is far from being so when the series is numerous. Last summer I constructed an apparatus of one hundred pairs, each containing six alternated plates, three of each metal. On trial, it proved much less powerful than the Deflagrator sent to you, though the zinc surface in each pair was one seventh larger, and the number of the series one fourth more extensive. The exposure to each other, of the copper and zinc plates terminating the different pairs, struck me as disadvantageous. I therefore removed the external zinc plate in each, so that the pairs afterwards, consisted severally of three copper and two zinc plates, and were bounded by copper towards both poles. There was some comparative gain by this change, as the power was not lessened in proportion to the diminution of zinc surface. Still the result was unsatisfactory. I then had some bottomless boxes made, furnished with glass partitions, to be interposed between the pairs of the series. These were employed as is usual with galvanic troughs, made with partitions, excepting that instead of containing the acid, they were suspended to the beams, so as to be simultaneously immersed with the galvanic surfaces which they were intended to insulate. The power of the series

demonstrated, not only by my own apparatus, but by those constructed by Messrs. Wetherill and Peale, and which were larger, but less capable of exciting intense ignition. Mr. Peale's apparatus contained nearly seventy square feet, Mr. Wetherill's nearly one hundred, in the form of concentric coils, yet neither could produce a heat above redness on the smallest wires. At my suggestion, Mr. Peale separated the two surfaces in his coils into four alternating, constituting two galvanic pairs in one recipient. Iron wire was then easily burned and platina fused by it. These facts, together with the incapacity of the calorific fluid extricated by the calorimotor to permeate charcoal, next to metals the best electrical conductor, must sanction the position I assigned to it as being in the opposite extreme from the columns of De Luc and Zamboni. For as in these, the phenomena are such as are characteristic of pure electricity, so in one very large galvanic pair, they almost exclusively demonstrate the agency of pure caloric."

was not amended by this contrivance. It had often occurred to me, that surrounding the zinc by copper, might be an indispensable feature in the arrangement of my Deflagrator of coils. In order to test the correctness of this surmise, I proceeded to form an apparatus of pairs, each consisting of a case of copper, containing one zinc plate of seven inches by three, the size used in the apparatus above described. (See the plate at the end.) In these pairs, as in those contrived by Wollaston, the edges of the zinc were supported by grooved pieces of wood passing between them and the copper. There was, however, this apparently slight, but really important difference, that the cases employed by me, were open at top and bottom, instead of exposing the edges of the zinc laterally, as in Wollaston's. One hundred galvanic pairs, thus made, were suspended to two beams, each holding fifty. Between each case, a piece of pasteboard soaked in shell lac varnish, was interposed; so that the whole constituted a compact mass, into which a fluid could not enter, unless through the interstices purposely preserved between the copper and zinc. The phenomena produced by this apparatus, on immersion, were upon the whole more interesting than those produced by my original deflagrator; especially in the length of the jet between the poles, and the power of permeating charcoal. Yet the apparatus was comprised within one eighth of the space, and is not (in oxidizable superficies) of half the extent.

Having added three more beams, of fifty pairs each, to my apparatus, I found the power increased fully in the ratio of the number. You know that my eyes are naturally very strong. The light produced by the compound blowpipe,* though I operated without glasses, only dazzled them for a time, and hitherto I had felt no other inconvenience from my galvanic experiments. Rendered thus bold by previous immunity, I still dispensed with the annoyance of spectacles. In consequence, my eyes, after operating with the last mentioned series of two hundred and fifty, were on the following

* Since called Hydro-oxygen Blowpipe.

day so much inflamed, as to be blood shot, and painfully susceptible of the day light. The judicious application of twenty leeches to each of the eye-lids, pursuant to the advice of my friend, Dr. Dewees, afforded me surprising relief, and my eyes are now well enough to finish this letter, though a few days since, when I began it, I was under the necessity of employing an amanuensis.

By this series of 250, barytes was deflagrated; and the platina which supported it destroyed like pasteboard before an incandescent iron. A platina wire three sixteenths of an inch in thickness, was made to flow like water. Iron of like dimensions burned explosively. When the experiments were repeated before my class of more than three hundred pupils, and many visitors, there were very few who could bear the light with the naked eye.

Much attention was excited by the deflagration of a stream of mercury. This was accomplished in the following way. A wire proceeding from one pole of the deflagrator, was introduced into some mercury held in a glass basin; and another wire proceeding from the other pole, into some mercury in another vessel, having a capillary orifice which might be closed by the finger or a stopple. This last mentioned vessel with the mercury running from it was supported at such a height above the surface of the mercury in the glass basin, as to permit the discharge to take place through the metallic stream just as the galvanic surfaces were subjected to the acid. The mercury deflagrated explosively.

The experiments may be varied, by causing the stream of mercury to fall on iron filings, or card teeth.

When the phenomena of a series of 250 pairs of 7 inches by 3, are such as I have described, what would be the power of a deflagrator with plates, as large as Children's, and as numerous as Davy's?

Probably the most useful mode of applying such instruments to analysis, would be to expose substances on carbon to the discharge in vacuo. I observed that after iron and charcoal were ignited between the poles during a few

seconds, under an exhausted receiver, on admitting the air, a flash took place, and a yellowish red fume appeared which condensed on the glass. It would seem the iron was volatilized,* and that the admission of air oxidized the vapour.

A deflagrator of 250 or 300 pairs, is found to produce torture when applied for a short time to the back of the hand, and it is difficult for the sufferer to believe, that his skin has not been cauterized. One of my pupils showed me a slight excoriation, which he considered as arising from it, on the spot where the positive pole had touched him. Between the excitement of acid, and water, the difference of power in affecting the flesh, is far less than with metals, charcoal, or potash. Upon these substances, the excitement by water has no influence, but to the sensation is painful, though it may be borne longer, than when acid is used. Neither is the shock greater, in any sensible degree, at the moment of immersion, than afterwards. The effect upon the electrometer, is at least as great, with water, as with acid. Immediately over any of the most turgid veins, where the skin is tender, as on the back of the hand, will be found the greatest sensibility. The positive pole, is most capable of producing pain. This I had frequent opportunities of ascertaining, by the observations of those who, not knowing how to distinguish it from the negative pole, could not have been biassed in their opinion. Upon a common gold leaf electrometer, a deflagrator of 300 pairs will have no influence. I have constructed one by means of a bottle, a single slip of gold leaf, and a knob at right angles to it, supported by a screw, so as to be easily moved nearer to or further from the leaf. The wire from which the latter is suspended, passes through a cork in the neck of the bottle. The screw enters through a nut, cemented into a hole drilled on one side. When the wire which supports the leaf, is fastened to one of the poles, every time the screw is touched by the other, the leaf will strike the ball, provided the distance be very small, perhaps not greater than the tenth of an inch.

* And possibly the carbon too?

This result was obtained at a greater distance when the coils had been recently withdrawn from the acid, than when they are covered by it. I have known a piece of dry sealing wax, as big as a chesnut, without friction, to affect this electrometer as much as my largest deflagrator.

A magnetic needle was very powerfully disturbed by the deflagrator, under all its forms. The celerity with which the galvanic surfaces may be immersed in, or withdrawn from the acid, contributes much to economy, and to the ease of the operator in galvano-magnetic inquiries.

The prevalent notion, that the intense light and heat produced by galvanic action, are results secondary to electricity, the presence of which is at times only indirectly discoverable, the more surprises me; since it does not in the smallest degree, elucidate the primary operation, by which this principle is alleged to be evolved. According to some philosophers, the contact of the metals alone, according to others this contact accompanied by their solution, evolves electricity in quantity sufficient to extricate heat and light from a wire made the medium of transmission. They do not, however, explain why the electricity does not, according to all its known habitudes, rapidly escape through the water, as fast as generated, instead of proceeding from one plate to another, in order to pass off through a second portion of the same fluid. Would it not be more philosophical, to suppose that the heat and light result *directly* from the causes supposed to produce them *indirectly;* especially, as we actually see *them* in a high degree of intensity, while the characteristic agency of the principle, by which they are supposed to be produced, is but feebly perceived, or imperfectly demonstrated? In the case of a single galvanic pair, electricity has never been alleged discoverable, unless by the questionable assistance of condensers.

Besides, without supposing caloric and light to circulate from the apparatus through the conjunctive wire, those who consider them as material, will find it impossible to account for the durability of the ignition. If it be supposed that these principles are extricated from the metal, only by elec-

tricity passing through it, their repeated or incessant expenditure, ought sooner or later to exhaust the metal, and render it incapable of further ignition.

On this subject, especially, as connected with magnetism, and mechanical electricity, you shall hear from me again.

LETTER IV.

Yale College, New-Haven, April 9th, 1822.

My Dear Sir,

In my letter of Oct. 23d, 1821, addressed to you respecting the experiments which I had performed with your deflagrator, I mentioned the incompatibility which I discovered to exist between your apparatus and the common galvanic battery. I have recently repeated these experiments with some additions and variations, which I now take the liberty of stating to you.

In the trials made last October with your instrument, the coils were used without glasses, being immersed in a fluid, contained in a common recipient. In those recently performed, and which I shall now relate, the metallic coils were individually insulated, for they were immersed in the cylindrical glasses belonging to the apparatus, it being previously connected with the common galvanic battery by its proper poles as described in my former letter; the effects were, however, in no respect different from those before observed, so that the insulation of the coils appears to be a fact of no importance. In the first experiment the deflagrator being connected by its proper poles with a galvanic battery of 300 pairs of four inch plates cemented in mahogany troughs, and interposed between the two rows of the deflagrator, of forty coils each, lost all its power, and the effect produced was very much inferior to that of the battery alone, for in fact the spark was hardly perceptible.

The chemical or decomposing powers of the common galvanic battery, were also found to be suspended by the connexion—for the 300 pairs which usually decompose

water, salts, &c. with decisive energy, now produced in water scarcely a bubble of gas, and hardly affected dilute infusion of purple cabbage. The power of giving a shock was also destroyed by the connexion.

When the coils were raised out of the fluid and suspended only in the air, they acted as conductors of the power of the common battery, which now produced *all* its appropriate effects, although, even in this case, the galvanic influence appeared somewhat diminished, which would of course arise both from *the extent* of the conducting surface, and from the fact that a part of the substance, namely, the wedges of moist wood, interposed between the metals, was an imperfect conductor.

These experiments (including the former trials) were made with different combinations from 620 pairs down to 20, and were attended, uniformly, with the same result; viz. an almost entire suspension of the power of both instruments.

In one of the experiments, twenty-five pairs of the zinc and copper plates, six inches square, connected by slips of copper and suspended from a beam of wood, were immersed in a trough without partitions filled with an acid liquor, and the connexion being formed with the deflagrator, the power of the latter instrument was found to be completely destroyed—a similar result was obtained by a battery consisting of fifty *triads* of plates two inches square, each zinc surface being coated by a copper plate after the manner of Dr. Wollaston—the object of this arrangement was, to ascertain whether a battery, in which the arrangement of metals was similar to that in the deflagrator, would produce a result in any respect different from that of the common battery; the effect, however, was precisely the same. In most of the experiments the connexion of the poles was occasionally reversed. This circumstance, however, made no difference in the result; a feeble spark was obtained as before. Every thing tended to countenance the opinion *that the interposition of the common galvanic battery operated simply as an impediment—that it was completely inert in*

relation to the deflagrator, and the deflagrator in relation to it,—that the power of neither would pass through the other, and consequently that each was to be regarded, with respect to the other, simply as so much interposed matter, constituting a conductor more or less imperfect. To bring this conjecture to a decision, the number of interposed plates was constantly diminished, until the connexion was formed by no more than twenty pairs. In this state of things, the power of the deflagrator passed freely, although somewhat diminished. The connexion was now formed with smaller and smaller numbers of pairs; the activity of the deflagrator in the mean time rapidly increased, until the moment, when only one pair was employed (this pair being, however, like the others, immersed in an acid fluid,*) then there was no perceptible impediment, and the effect was as brilliant as when nothing was interposed.

I have thought these curious facts worthy of being preserved, and I have addressed them to you with the hope that you will be able to throw some light upon this singular anomaly, which to me appears to be incapable of explanation, in consistency with the received theories of galvanism. Hoping that you will, through the medium of this journal, favour the public with your views upon this subject,

I remain with very great respect,

Your friend and servant,

B. SILLIMAN.

To Professor Hare.

LETTER V.

Philadelphia, April 25, 1822.

My Dear Sir,

In a former letter you mentioned, that you had found the power of the galvanic deflagrator, when its coils were subjected to acid in troughs without partitions, incompatible with the power of other voltaic series, of the usual forms;

* When this *one* pair was surrounded by air alone, the power of the deflagrator passed freely, as might have been expected.

that when associated with them in one circuit, it could neither give, nor receive excitement. You now inform me, that this incompatibility is not lessened when the coils are insulated by glass jars.* It follows, that electrical insulation has less influence on the action of this instrument, than I had supposed, and it of course confirms my idea, that the deflagrating power is not purely electrical.

It cannot be doubted, notwithstanding your experiments, that there is a principle of action, common to the various apparatus which you employed, and all other galvanic combinations. The effect of this principle of action, however, varies widely according to the number of the series, the size of the members severally, and the energy of the agents interposed. Towards the different extremes of these varieties are De Luc's column apparently producing pure electricity, and one large galvanic pair, or calorimotor of two surfaces, producing, in appearance, only pure caloric. At different points between these, are the series of Davy and Children; the one gigantic in size, the other in number. In the deflagrator we have another variety, which, with respect to size and number, is susceptible of endless variation.

It must be evident that no galvanic instrument, where a fluid is employed, could aid, or be aided by, the columns of De Luc or Zamboni. Nor could the influence of either be transmitted by the other. A calorimotor could not aid Davy's great series; nor could the latter act through a calorimotor. Taking it for granted that there can be no oversight in your experiments, this incompatibility of exciting power must exist to a great degree, under circumstances where it could hardly have been anticipated.

Were the fluid evolved by galvanic action purely electric, the effect of batteries of different sizes, when united in one circuit, ought not to be less than would be produced if the whole of the pairs were of the smaller size. But if on the

* See Memoir and engraving in Silliman's Journal for February, 1821. Also, in Tilloch's Magazine, and the Annals of Philosophy, for April and May, 1821.

contrary, we suppose the voltaic fluid compounded of caloric, light and electricity, so obviously collateral products of galvanic action; the ordinary voltaic series, employed in your experiments, may owe its efficacy more to electricity—and the deflagrator more to caloric. The peculiar potency of both may be arrested when they are joined, by the incompetency of either series to convey any other compound than that which it generates. The supply of caloric from the ordinary series may be too small, that of electricity too large; and vice versa. It might be expected that each would supply the deficiency of the other; but it is well known that many principles will combine only when they are nascent. The power of my large deflagrator (described in letter II.) in producing decomposition, is certainly very disproportional to its power of evolving heat and light. When wires proceeding from the poles were placed very near each other under water, it was rapidly decomposed; but when severally introduced into the open ends of an inverted syphon, filled with that fluid, little action took place. Potash is deflagrated, and the rosy hue of the flame indicates a decomposition; still however the volatilization of the whole mass, and intense ignition of the metallic support, prove that the calorific influence is greatly and peculiarly predominant.

I fear that in my essays on galvanic theory, the possible activity of light has been too much overlooked. The corpuscular changes which have been traced to the distinctive energies of this principle are so few, that we have all been in the habit, erroneously perhaps, of viewing it as an inert product in those changes, effected by caloric, electricity, and chemical action, which it most strikingly characterizes. Yet, reflecting on the prodigious intensity in which it has been extricated by the Deflagrator, it seems wrong not to suspect it of being an effective constituent of the galvanic stream. Possibly its presence in varying proportions, may be one reason of the incompatibility of the voltaic current as generated under different circumstances, or by various forms of apparatus. It may also suggest, why in addition to changes in the force or nature of the sensation produced by the gal-

vanic discharges which may be considered as dependent on electric intensity, peculiarities have been observed, which are not to be thus explained. The effect on the animal frame, has been alleged to be proportional to the electrical *intensity*, the effect on metals to the *quantity;* but according to the observations of Singer, (which are confirmed by mine) the electrical intensity is as great with water as with acid, if not greater even than with the latter. The reverse is true of the shock. When the plates of the deflagrator are moistened, and withdrawn from the acid, the shock is far less powerful; yet the electrical excitement appears stronger. Light is undeniably requisite to vegetable life, perhaps it is no less necessary in the more complicated process of animal vitality, and the electric fluid may be the mean of its distribution. The miraculous difference observed in the properties of organic products, formed of the same ponderable elements, may be due to imponderable agents conveyed and fixed in them by galvanism. Hence it may arise, that the prussic acid instantaneously kills when applied to a tongue, containing the same ponderable elements. When by the intense decomposition of matter, light is always evolved; when an atom of tallow gives out enough of it to produce sensation in the retina of millions of living beings, why may it not when presented in due form, influence the taste, and otherwise stimulate the nervous system. For such an office its subtilty would seem to qualify it eminently. The phenomena of the fire-fly and the glow-worm prove that it may be secreted by the process of vitality.

The discovery of alkaline qualities, as well as acid, in organic products whose elements are otherwise found, whether separate or in combination, without any such qualities, and the opposite habitudes of acids and alkalies with the voltaic poles, and their power of combining with, and neutralizing each other, indicate that there may be something adventitious which causes alkalinity and acidity, and that this something is of an imponderable character, and dependent on galvanism.

In the number of your Journal for October last, I gave my reasons for believing in the existence of material imponderable principles, producing the phenomena of heat, light, and electricity. The coexistence of these principles in the medium around us, their simultaneous or alternate agency and appearance, during many of the most important processes of nature, seem to me to sanction a conjecture, that as ingredients in ponderable substances, they may cause those surprisingly active and wonderfully diversified properties usually ascribed to apparently inadequate changes, in the proportions of ponderable elements.

In obedience to your request, I have thus displayed the ideas at present awakened in my mind by these obscure and interesting phenomena. I am not willing to assume any responsibility for the correctness of my conjectures. Possibly they may excite in you farther and more correct speculations.

LETTER V.

FUSION OF CHARCOAL, *by the Deflagrator, with proofs of a current between the Poles.*

Yale College, New Haven, May 10, 1822.

My Dear Sir,

IN your memoir on your Galvanic Deflagrator, (p. 110, Vol. III. of my Journal,) when speaking of the ignition produced by that instrument, in charcoal points, you remark: "If the intensity of the light, did not produce an optical deception, by its distressing influence upon the organs of vision, the charcoal assumed a pasty consistence, as if in a state approaching to fusion.

"That charcoal should be thus softened without being destroyed by the oxygen of the atmosphere, will not appear strange, when the power of galvanism in reversing chemical affinities is remembered; and were it otherwise, the air could have no access; first, because of the excessive rarefaction,

and in the next place, as I suspect, on account of the volatilization of the carbon, forming about it a circumambient atmosphere. This last mentioned impression arose from observing, that when the experiment was performed in vacuo, there was a lively scintillation, as if the carbon in an aeriform state, acted as a supporter of combustion on the metal."

This paragraph, at the time of perusing it, excited in my mind a lively interest, and a strong wish to see so fine a result as the fusion of charcoal, confirmed by an experiment admitting of no question. What you threw out by way of surmise, and without positively affirming it, I think I am now able to substantiate.

During the three last weeks of March, I was much occupied with your deflagrator. The medium of communication between the poles, was generally charcoal prepared for the purpose, by intensely igniting pieces of very dry mahogany, buried in a crucible, beneath white siliceous sand. The pieces of charcoal thus prepared, were about half an inch in diameter, and from one and a half inch, to three inches in length; they were made as usual, to taper to a point, and the cylindrical ends were placed in the sockets, connected with the flexible lead tubes, which form the polar terminations of the series.

The metallic coils of the deflagrator being immersed, on bringing the charcoal points into contact, and then withdrawing them a little, the most intense ignition took place, and I was surprised to observe, that the charcoal point of *the positive pole*, instantly *shot out*, in the direction of the longer axis, and thus grew rapidly in length; it usually increased from the tenth to the eighth of an inch, and in some instances attained nearly one fourth of an inch in length, before it broke off and fell. Yesterday and to-day, I have carefully repeated these experiments, and in no instance has this shoot from the positive pole failed to appear. It continues to increase rapidly, as long as the contiguous points of charcoal are held with such care, that they do not strike against each other. When they impinge with a slight shock, then the projecting shoot or knob breaks off and falls, and is in-

stantly succeeded by another. The form of the projecting shoot is sometimes cylindrical, but more generally it is that of a knob, connected with the main piece of charcoal by a slender neck, much resembling some stalagmites. It is always a clear addition to the *length* of the charcoal, which does not suffer any waste except on the parts *laterally* contiguous to the projecting point.

The charcoal of the negative pole, in the mean time, undergoes a change precisely the reverse. Its point instantly disappears, and a crater-shaped cavity appears in its place; it suffers a rapid diminution in the direction of its length, and immediately under the projecting and increasing point of the positive pole; but it is not diminished, or very little, on the parts laterally contiguous. If the point of the positive pole be moved over various parts of the contiguous negative charcoal, it produces a crater-shaped cavity over every place where it rests, for an instant. In every repetition of the experiment, (and the repetitions have been numerous,) this result has invariably occurred. *It appears as if the matter* at the point of the negative pole was actually transferred *to the positive, and that the accumulation there, is produced by a current, flowing from the negative to the positive, or at least by an attraction exerted in that direction, and not in the other.* It does not appear easy to reconcile this fact with any electrical or igneous theory.

In order to ascertain whether the projection of the charcoal at the positive pole was caused by an actual transfer of carbon from the negative, a piece of metal was substituted for the charcoal at the negative pole, and when the two were brought into contact, the charcoal point of the positive pole remained unaltered in form, although a little shortened by the combustion. The experiments with the two charcoal points were varied by transferring that at the positive end, (and on which a projection was already formed) to the opposite pole, and that at the negative, and in which a corresponding cavity appeared, to the positive.

The result was, that the cavity now placed at the positive pole, disappeared, and was immediately seen at the nega-

tive; while the projection, now placed at the negative pole, was transferred to the positive. These experiments were several times repeated, and uniformly with the same result. They seem to leave no doubt, *that there is a current from the negative to the positive pole, and that carbon is actually transferred by it in that direction;* if transferred, it must probably be in* the state of vapour, since it passes through the ignited arch of flame, which is formed when the points are withdrawn a little distance; when it arrives at the positive pole, it there concretes in a fluid, or at least in a soft or "pasty" state.

But the most interesting thing remains yet to be stated. On examining, with a magnifier, the projecting point of the positive pole, it exhibited decisive indications of having undergone *a real fusion.*

The projecting point or knob, was completely different from the charcoal beneath. Its form was that of a collection of small spheres aggregated; exhibiting perfectly, what is called in the descriptive language of mineralogy, botryoidal or mamillary concretions. Its surface was smooth and glossy, as if covered with a varnish; the lustre was metallic, the colour inclining to grey, exhibiting sometimes iridescent hues, and it had entirely lost the fibrous structure. In short, in colour, lustre, and form, the fused charcoal bore the most striking resemblance to many of the beautiful stalactitical and botryoidal specimens of the brown hœmatite. The pores of the charcoal had all disappeared, and the matter had become sensibly harder and heavier.

I repeated the experiments, until I collected a considerable quantity of these fused masses; when they were placed contiguously, upon some dark surface, with some pieces of charcoal near them, they appeared when seen through a magnifier so entirely different from the charcoal, that the

* Those who would contend for a current in the opposite direction, would probably say, that the projecting point of the positive pole, is formed from the carbon, contiguous on the sides, and that the stream of heat burns the cavity in the opposite pole; *in either way a current is proved.*

would never have been suspected to have had any connexion with it, had it not been, that occasionally some fibres of the charcoal adhered to the melted masses. The melted and unmelted charcoal, differ nearly as much in their appearance as pumice-stone and obsidian, and *quite* as much as common stones do, from volcanic scoriæ, excepting only in the article of colour. It is to be understood, that the examination is, in every instance, made by means of a good magnifier, and under the direct light of the sun's rays, as the differences are scarcely perceptible to the naked eye, especially in an obscure light. The portions of melted charcoal are so decidedly heavier than the unmelted, that when fragments of the two of a similar size are placed contiguously, the latter may be readily blown away by the breath, while the former will remain behind; and when the vessel containing the pieces is inclined, the melted pieces will roll with momentum, from one side to the other, in a manner very similar to metallic substances, while the fragments of charcoal will either not move, or move very tardily.

It should be observed, that during the ignition of the charcoal points, there is a peculiar odour, somewhat resembling electricity, and a white fume rises perpendicularly, forming a well defined line above the charcoal. There was also, a distinct snap or crackling when the two points were first brought together.

Wishing to ascertain whether the alkali, present in the charcoal, had any effect in promoting the fusion, some pieces of prepared charcoal were thoroughly boiled in water, and were then again exposed to a strong heat in a furnace beneath sand in a crucible. These pieces, when connected in the circuit, exhibited the same appearances as the other, and proved equally fusible.

Without destroying cabinet specimens, I could procure no diamond slivers, and have not, therefore, attempted the fusion of the diamond, which must be left to another opportunity. Our circle of fusible bodies, so much enlarged

by the use of your instruments, is now so nearly complete, that it would be very desirable to fill the only remaining niche, namely, that occupied by plumbago anthracite, and the diamond.

I remain as ever, truly your friend and servant,

B. SILLIMAN.

P. S. I do not suppose, that those who repeat these experiments, will succeed with the common galvanic apparatus. I deem it indispensable, that they be performed with the *deflagrator*, and with one equal in power to mine.

We here insert, in succession, three different accounts of a very extraordinary disease, which prevails in certain sections of our country. The first is an original communication, and the others are extracted from the "Western Quarterly Reporter," a medical journal recently instituted at Cincinnati, of very high promise. Each of these writers ascribes the disease to the eating of the milk, or flesh of animals, become vitiated by their having fed on a poisonous plant, not hitherto accurately ascertained. The third number of this Journal contains a paper on the same subject by Dr. Lea, a most intelligent physician of Tennessee, who, we think, refutes this hypothesis, or at least, renders it more probable, that it is owing to a pestiferous miasm, a sort of *malaria*. That exhalations of this nature are exceedingly varied by local and other circumstances is now well known, and much evidence is adduced by him to show that such modification has occurred in the case before us. The question, however, of the origin of the disease, may still be considered *sub judice*, and as an extremely interesting one it calls for a further and more precise examination. We shall be glad to receive communications on it, and

indeed concerning the diseases generally which are, in any degree, peculiar to the western country.

EDITOR.

ART. VIII. *An Account of the Diseased State produced by taking into the Stomach the Milk or Flesh of Cows, which has acquired deleterious properties from a Vegetable sometimes eaten by those Animals.* By ALEXANDER M'CALL, M. D., of Tennessee.

THE fertility of the soil adjacent to Goose creek—the luxuriancy of vegetation—the numerous beautiful situations for buildings and farms—with the vicinity of the large navigable Cumberland river, all contributed to invite to this place many of the early adventurers to Tennessee. The rapid advancement made in agriculture, and the great increase of domestic animals, soon proved destructive to the arundinaceous growth which overspread the whole face of the country, and a variety of small herbage in a short time supplied its place. Among other plants is one called by the country people "*Indian Hachy*," which is said to have been used medicinally by the Cherokee Indians for some purpose. But it is now chiefly remarkable for imparting to the milk of cows that feed on it, certain properties highly pernicious to the health of animals when taken into their stomachs. This plant, the botanical title of which I do not know, is perennial, sprouting up early in the spring, and flowering in July: its blossoms are of a bluish colour, fixed closely to an upright stamen of three or four inches in length. The root is somewhat bulbous, and the leaves bear a slight resemblance to the arum triphyllum when young and tender. So well convinced are the inhabitants of Goose creek that this is the particular plant possessed of such highly detrimental qualities, they have fenced in all those places where it is known to grow, with a view of preventing the access of cattle to it.

Notwithstanding this precaution, some persons annually fall victims to this deleterious plant. Noxious indeed must be its properties, since the very small quantity of

milk usually taken in tea, when imbued with it, has produced the worst consequences. Travellers passing through the country are said to be particularly liable to feel the effects of using this milk in their diet. The susceptibility of the inhabitants is, perhaps, in some degree lessened, by being often subjected slightly to the impressions of the poison.

After swallowing the milk, the person in a short time suffers from thirst, nausea, vertigo, confused or imperfect vision, vomiting often ensuing, succeeded by a violent fever, the exacerbations subsiding at irregular intervals. The pulse is extremely variable, sometimes strong and full, at others, tremulous, small and corded. Constipation, which exists from the beginning, becomes more obstinate towards the third or fourth day. The skin, also, about this time is more hot and parched—the eyes are red and suffused—there is very great restlessness, and all the secretions are scanty. Towards the sixth or seventh day excessive debility takes place, with very often paralysis of the tongue and other parts, and soon after, ensue stupor, cold clammy sweats, convulsive hiccough, an offensive cadaverous odour, and death.

Cases of a very mild form, after a continuance of six or eight weeks, have ended fatally. But in general the patient is considered safe if he survive the eleventh day.

In the year 1820 the citizens of Hartsville, a small village near Goose creek, were almost all made sick by partaking of beef sold in their market.

Amongst others were a tailor, his wife and son. Though all the usual remedies were resorted to for the relief of these persons, the latter only survived. This boy I saw some months after his attack, at the house of one of his relatives. He was much emaciated, his abdomen tumid, feces of a greyish colour, legs and feet somewhat œdematous—complained of pain in the head, and of dimness of sight. Not the least singular circumstance attending this case was a cutaneous affection, consisting of a number of distinct flattened pustules in different states of maturity.

The largest of these pustulous risings might have been covered by a shilling—and when in their forming stage, a dark grumous fluid issued from them by a slight pressure of the finger.

Dogs, cats, hogs, buzzards, crows, or indeed any animal eating of the vitiated milk or flesh, are affected in a similar manner, and generally die in a few days.

I saw a dog pass through every stage of the disorder. He had fed on the flesh of a calf which died by sucking its mother's milk. The dog coming home with some putrid flesh upon him, I suspected what he had eaten, and watched him. He appeared to be in a state of great anxiety—went to a spring for water, and then returned to the poisoned carcase. Calling him, he came to me manifestly very sick. On the way, he eat some grass and vomited. After getting home he concealed himself under a floor, where he lay all the ensuing day. His eyes became fiery red, and a peculiar hoarseness marked his barkings. He remained in the same condition next day, refusing every kind of sustenance except water. His whinings were almost continual, and he seemed intent on biting whatever came within his reach. The next morning I found him dead.

It may here be worth mentioning that an opinion prevails among some, that this morbid state of the system in the human species especially, has a strong tendency to return annually for some years, about the same season it was originally contracted, with symptoms, however, much mitigated on each recurrence. Whether there is any foundation for such an opinion I cannot say from my own knowledge. As somewhat analogous, the fact might be brought forward in its support, that persons poisoned, and particularly by the rhus radicans, unless completely cured, are liable to similar repetitions of attack. It is also well known that persons who have suffered severely from the bite of a poisonous snake, will in some instances feel the bad effects periodically.

I regret very much that I never procured any of the poisoned milk or meat for the purpose of experimenting with

it. As yet, so far as I know, there has been no attempt to examine the properties of this milk by analysis, or of the vegetable from which it is said to obtain its pernicious qualities.

An inquiry into the pathology of this disease, would involve some theoretical points about which there has been much dispute. This consideration alone, even if my brief limits allowed of it, would prevent me from engaging, at present, in such a discussion.

In treating the complaint, it has been found advisable always to administer an active emetic as soon as it is ascertained that poisoned milk or meat has been taken into the stomach. Great relief is afforded by the free use of the lancet during the early stage of the fever. Charcoal and spirit of turpentine early exhibited, are considered very beneficial. Laudanum has been given to relieve pain in the region of the stomach. But sinapisms and blisters applied over the chest and epigastrium, are said to effect this purpose more promptly.

Cold water poured on the extremities, and stimulating enemata of different compositions, conjointly with purgatives of the most active kind, have been resorted to, with a view of arousing the intestinal canal from its torpid condition. This is the outline of the practice commonly pursued.

In closing this communication, I have to regret, that my knowledge of the subject, has not enabled me to afford more ample and satisfactory information on it. But hereafter, I mean to pursue the inquiry with greater care and diligence, and shall be happy to lay before the public, an account which I hope will have stronger claims to attention.

ART. IX.—*Observations on the Disease generally known by the name of the Sick Stomach.* By DR. ASA COLEMAN. Communicated by D. DRAKE, M. D.

[From the Western Quarterly Reporter.]

A DISEASE has been prevalent in some of the western counties of the state of Ohio, since their first settlement, which is singular in its symptoms and frequently fatal in its effects. The country people name it the Sick Stomach, from the sickness and vomiting attending it. This disease is common to a certain extent on the MIAMI RIVER and all its branches, north of Dayton, particularly on Mad River, and the open country between this and the Scioto river. Some other parts of the Western Country are also subject to it.

This malady appears to be, in a great measure, confined to particular tracts of country, often of no great extent, and in which there is no peculiarity as to humidity, dryness or elevation, different from the adjoining country where it is not met with; nor does it appear to extend its geographical limits since the part of the country in question has become generally settled. The time at which it is most common, is that of the summer and autumn, though it is occasionally seen in all the other seasons.

In narrating the symptoms, we shall divide the disease into three stages or forms, to render the description more perspicuous.

First stage.—The patient complains of a general weakness and lassitude; pain or soreness in the calves of the legs, sometimes extending up the thighs; disagreeable sickening sensations are felt at the stomach occasionally; the appetite becomes somewhat impaired; sometimes a disagreeable gnawing sensation in the stomach is complained of, especially if food be not taken at the usual time. This sensation appears to be less after eating. He is now easily

exhausted by exercise, especially from walking, which increases the pain in the calves of the legs, and produces so much loss of muscular power that the limbs are not able to support the body, and the patient is compelled to rest. This condition is for the time ameliorated thereby, but will return on a repetition of the exercise, and if this is violent the disease assumes a more active form. These symptoms will sometimes continue for weeks, or months, and disappear without the more active forms of the disease succeeding, while, under other circumstances, they quickly ensue with much violence. During the first stage of the disease, the pulse differs but little from the healthy state. It is sometimes rather weak, and slower than natural, or it is quick and tense. The bowels are generally regular.

Second stage.—These symptoms sometimes continue for a few days only, especially if the patient has taken much exercise. What I term the second stage, then begins by an aggravation of the preceding symptoms. The great loathing, nausea and disagreeable sensation are much increased, and soon succeeded by vomiting or distressing retching to vomit, without throwing much off the stomach.—The vomiting often returns every hour or two during a whole week together, and the patient experiences some relief after each effort. During this stage of the disease, the strength is much prostrated; the patient complains of great distress and a burning sensation at the stomach; the bowels become obstinately costive; the thirst is frequently great; hot belchings and hiccough are often troublesome; the tongue is slightly furred, and the breath has a peculiar, disagreeable smell, resembling that of a person under a mercurial course.

The countenance is generally somewhat flushed, and the patient inclined to be stupid and comatose. There is often dyspnœa, and the respiration is quite laborious.—The pulse is slower than natural—weak—easily compressed—becoming more frequent and somewhat tense about the third day from the commencement of the vomiting. The extremities are generally inclined to be cold, and the skin is generally as cool as natural, and dry.

The vomiting will often continue six or eight days, the patient remaining unable to take any solid food, and liquids are speedily thrown from the stomach.—As the patient is somewhat relieved after each spell of vomiting, he falls into a doze. These symptoms will continue, in favourable cases, from the seventh to the ninth day, with greater or less violence, when they suffer some abatement, and the patient is able to retain some food on the stomach, and has generally a very speedy convalescence. Sometimes the disease, instead of taking the above-mentioned course, assumes symptoms of a more fatal character, which I shall describe as the third stage or form of the disease.

Third stage.—About the third or fourth day from the commencement of the vomiting, the patient experiences a slight pain in the head; for a short time becomes highly delirious, with continual tossing and moaning. The pulse is rather tense and frequent; the eyes protruded, suffused, sometimes the pupils are dilated; the patient manifesting many symptoms of true phrenitis. In such cases, the vomiting is often not so great, nor the bowels so obstinately constipated. When these symptoms arise, they are apt to destroy the patient in three or four days, if not sooner. In other cases, about the third or fourth day, the patient will fall into a very stupid comatose situation, with a feeble pulse, and symptoms appearing to denote that a gangrene of the stomach has ensued. Such are, the cessation of the distress of the stomach, with an occasional vomiting of black, porraceous matter. After the appearance of these symptoms, the patient is generally destroyed in a day or two. Such are the general symptoms and causes of this disease, when not interrupted by medical treatment, which produces variations to a greater or less extent.

The propensity to sleep is often destroyed for several days after an attack of the active form of this disease, and an uncommon degree of wakefulness is experienced. No chilliness occurs at the onset of this complaint, neither during its course, nor does it appear subject to any thing like periodical exacerbations, and in general there is no re-

markable indication of superabundance of bile. In some few cases, however, there is a considerable discharge of green bilious matter by vomiting, though generally, the material thrown out of the stomach consists of liquids that have been swallowed, which are often hot, acrid, or sour when ejected. From the throat and fauces there is commonly a discharge of thick viscid phlegm.

From imprudent exposure, those who have had this disease in its active form, are liable to relapses, but perhaps not more so, than in our common autumnal diseases. The patient usually convalesces with speed, seldom requiring tonic medicines.

This affection attacks indiscriminately both sexes, and all ages and conditions; though it is more severe upon middle aged adults, than old persons or children. The degree of danger is to be estimated by the violence of the symptoms, especially from the presence of such as denote gangrene of the stomach, or great disturbance of the brain. Though the hiccough is often a very troublesome and distressing symptom, yet it does not point out any particular danger. The first stage seems to be a kind of chronic condition, and does not continue for any definite period. The more active forms of the complaint are very regular in duration, and seldom exceed the ninth, or stop short of the seventh day, unless in cases of fatal termination. The mortality from this disease is perhaps not so great, as might be expected from the nature of the symptoms. About one case out of twenty or thirty is lost, of those who experience the disease in its active form.

Medical Treatment.

In the first stages, an occasional use of emetics, cathartics, and laxatives, with rest and the use of light, wholesome food, will generally clear the system from its effects in a few weeks. In the more active forms of the disease, the exhibition of medicine is often extremely difficult, owing to the great irritability of the stomach. Emetics are then

absolutely hurtful, especially antimonials, because of the irritation they excite in the stomach. In some cases where there was much retching to vomit, without throwing much up, I have directed a strong infusion of chamomile flowers, or warm water, to clear the stomach of its foul contents, (with great advantage.) Cathartics especially of the saline kind, afford considerable relief, if the stomach will retain them sufficiently long to permit them to act on the bowels. It is generally necessary to assist their operation by the use of stimulating enemata, in order to overcome the obstinate costiveness. These remedies must be repeated daily, during the continuance of the disease. Before resorting to the use of cathartics, however, I generally have recourse to some medicine for the purpose of allaying the irritability of the stomach, and to check the vomiting until the cathartic has time to operate. This is often difficult to accomplish. For this purpose I have found a solution of a few grains of carbonate of potass in water, very serviceable, when often repeated; sometimes I have added a few drops of tincture of opium. The sugar of lead is another remedy that I have seen advantageously used to allay the vomiting till the operation of a cathartic could be produced. The operation of a cathartic generally affords a considerable alleviation of the distressing symptoms; a large blister over the stomach appears to be of much service; bathing the feet in hot water, and applications of mustard to the feet and wrists, are usually resorted to. The general warm bath has been employed in some instances with advantage.

Blood-letting has not been employed as a general remedy in this affection, nor does it appear essential to a cure in most cases: from my experience, I deem it of much greater consequence, than is generally apprehended. The pulse, from being weak and depressed because of the excessive vomiting, often becomes fuller and stronger, and the symptoms are relieved by blood-letting. In cases where the brain begins to suffer, it is the remedy on which I chiefly rely; patients have borne it to a great extent with manifest bene-

fit. When the burning sensation at the stomach is troublesome, I have sometimes given nitre in small doses.

When symptoms of gangrene of the stomach begin to make their appearance, the use of medicine is necessarily very uncertain. I have, however, seen two or three patients in whom there was a vomiting of black porraceous matter, with a weak fluttering pulse, and who were in a comatose state, recover by the free use of stimulants, such as wine, decoction of bark, camphor, and the external application of blisters, sinapisms, and stimulating enemata.

Remarks on the causes and nature of this disease.

There are some circumstances connected with the history of this disease, so peculiar as to make them quite interesting. It is now a generally received opinion in the parts of the country where it prevails, that it is produced by some peculiar poison of the vegetable kind, and that the human system is affected with the disease only in consequence of using food that has been under the influence of this poison, such as milk, butter, or the flesh of animals labouring under the effects of this disease. The idea of a disease so formidable, produced in this manner, may appear somewhat chimerical to some, but there are many circumstances that favour the conclusion, and leave little doubt in my mind of its being the fact.

1st. That domestic animals become affected with this disease, and under certain circumstances only, and that they are not subject to it except in parts of the country where the human species are subject to it. That cattle become affected with the disease by eating some vegetable poison, is inferred from these circumstances; cattle in pasture fields where the ground has been cultivated, are not subject to it, it being only those that run at large in the woods and commons. I will state one of the many instances that have fallen within my observation, in support of this opinion: A piece of woodland was enclosed as a sugar orchard, in which there was no water, either running or stagnant; situation

high and dry; if an horse or ox were suffered to graze in this enclosure, for twelve or twenty-four hours, it would certainly become affected, and perhaps die in a day or two; while cattle in a pasture field adjoining, have grazed the whole season without the least symptoms of it. It may be inferred from circumstances like the above, that it is not the effect of impure water. Many instances have been related to me by persons of credibility, of horses escaping from the stable and not remaining in the woods more than twelve or twenty-four hours, becoming so much affected as to die in a day or two, having all the peculiar symptoms of this disease in brutes, and this often in the winter season, when the ground was covered with snow; from which circumstance it is suspected to be a vine, or some plant not easily affected with the frost. The disease is most fatal among cattle and horses in the fall months, but they become affected with it at all seasons of the year when turned to the woods. Cattle and horses appear subject to what I have termed the chronic form of this disease, and will show symptoms of it for weeks or months after being removed out of the limits of the supposed cause, and will also thrive well and get fat if the affection is but slight, and they are not subject to much exercise.

Cattle and horses do not always become affected with this disease, if allowed to range in the woods where the disease is common; some appear to possess sufficient instinct to avoid the poisonous vegetable. Cattle that have been raised in pasture fields, or in a distant country, appear most liable to become affected with it when turned to graze where the disease is common.

2d. That the milk, butter, beef, or the flesh of any animals, killed while labouring under this disease, will produce disease again in other animals, is proved by daily experience; sucking calves, which have no food but the milk of an affected cow, will show the peculiar symptoms, and often die of the disease; persons making use of the milk or butter, from the same cow, at the same time, will become affected.

The milk given to domestic animals when the sucking calf shows symptoms of it, will produce the disease upon them, and this often without the cow's appearing much affected by the disease, a milch cow seldom showing many symptoms of it when regularly milked; the poison appearing to pass off principally by that secretion.

I saw an instance of a whole family becoming sick with this disease, some of them in a few hours after dining upon a loin of veal, in which it was afterwards satisfactorily ascertained that the calf laboured under the disease at the time it was butchered, being sold in market by an unprincipled person. I well recollect of an instance of several persons becoming severely affected, from incautiously eating of a pig, which had been fattened upon the milk of a cow that was known to be affected with this disease; all of the family who ate of it, were seized, some of them in a few hours: there were circumstances attending this case, admitting no doubt of the source from which it originated. The effects of this disease on hogs and dogs, are often severe, and a subject of very common observation in parts of the country where the disease is prevalent; they will often, after feeding upon the carcase of an horse or ox that has died of this disease, become affected and die, manifesting similar symptoms as the animal did of which they had eaten; even the wolf has been seen near a carcase, unable to make way to his shelter, with evident marks of this disease.

Mr. Thomas Hill, a respectable farmer living near the southwest branch of the Miami river, informed me that having an ox die of this disease in his yard, and neglecting to remove it, his hogs, about seventy in number, devoured the carcase, and out of the above number about forty died in the course of twenty-four hours; the hogs were in good order, and appeared to be healthy previous to their feeding upon the carcase of the ox.

It will doubtless be asked, if it be a vegetable poison that produces all these alarming effects, why has not the particular vegetable been pointed out, and its qualities ascertained, especially as its effects upon animals are different from those

of any with which we are acquainted? I can only answer, that I have taken much pains, and tried some experiments, upon plants that have been supposed to be the cause, but have been as yet unsuccessful in finding any one that would produce the peculiar symptoms of this disease. Perhaps a strong evidence of the origin of this disease being in some poisonous vegetable, may be drawn from the total exemption of those persons from it, who are particular as to the meat, milk, butter and cheese they make use of, in situations where the disease has been most common; cattle and horses are also perfectly exempt from it while kept in pasture fields where the ground has been cultivated, or in the stable. By attention to these circumstances, farmers have a sure way of avoiding it in their persons and cattle: were not this the case, some very fine tracts of country would be difficult to inhabit, from the prevalence of the disease.

This disease is perhaps not dissimilar in many respects to diseases produced by some of the vegetable poisons of the narcotic class, with which we are acquainted, a large portion of it producing stupor, nausea, anxiety and vomiting, with other marks of an inflammation of the stomach; sometimes vertigo, pain in the head and delirium, where the disease is violent; the inflammation of the stomach perhaps is most generally of the erythematic kind, sometimes evidently of the phlegmonous. As to the particular modus operandi of this supposed vegetable poison in producing the constitutional affection which evidently takes place in what I have termed the chronic form of this disease, as well as in its active form, I shall not attempt to explain or hazard an opinion upon.

The foregoing remarks will apply to the disease described, as it has appeared for several years past. The last season it has been much more prevalent than usual, and unusually severe in its symptoms. It is now the middle of January, 1821, and the complaint is still common, and unusually fatal. The uncommon prevalence of the disease during the season, has been attributed to the unusual dryness of it, which produced a great scarcity of vegetable food for cattle,

thereby inducing them to eat any thing green, which has rendered it very fatal to them; and consequently, by inattention or scepticism as to the cause, has made it very prevalent and severe with the human species. The symptoms attending the disease the past season, have been more inflammatory than usual, and the affection of the brain greater, requiring early and copious blood-letting; some cases have terminated fatally in two or three days, from the active form of the disease supervening, where medical aid was not early called for, or improperly applied. The disease has been attended with a peculiar pain in the head, often with a stupor or coma, which, if not relieved by early blood-letting, has soon been followed by insensible stupor, or high delirium, and has soon destroyed the patient. In some cases a dilatation of the pupils of the eyes has been observable. A free use of the lancet, cathartics, epipastics, and the use of alkalies, has been the most successful mode of treatment.

Facts relative to the same Disease, By Dr. Haines, of Dayton.

This disease prevails chiefly in the neighbourhood of heavily timbered, level, and consequently rather wet oak land.

Cattle, sheep and horses, which range in these tracts, are subject to a peculiar disease known among the people by the name of the *Trembles*, which they suppose to be produced by their eating of a plant, as those animals which range in the neighbouring barrens or beech land, are free from the disease until they find their way into these low bottoms; and so well are the people acquainted with this fact, that if they see cattle that have been accustomed to range in the barrens, enter these suspected ranges, they foretell the sickness of the calves, and of the family using the milk. Calves are seized with trembling and vomiting during or soon after sucking, and frequently die of the disease. The milk has the same effect upon dogs. Children

drinking the milk, leave the table and vomit. Upon adults its effects seem to be less sudden, but eventually more severe. It seems somewhat singular that milch cows escape the disease, while their milk produces it in other animals. It seems as if the deleterious matter were eliminated from their systems through the lactiferous vessels.

Beef and mutton produce the disease. Several instances have been related to me, in which beef and mutton killed in the neighbourhood before mentioned, have sickened all who ate of them. Some who have had the disease, say they can instantly discover by a peculiar taste, such beef or milk as will produce the Sick Stomach. Dogs eating the flesh of those animals which die of the Trembles, as it is called, sicken and die with vomiting and that extreme debility characterized by trembling and inability to stand or walk.

If cows be kept in pastures and not suffered to run in the woods, their milk may be used with impunity. That there are difficulties in admitting the opinion that this disease is caused by some vegetable poison, and in proving it to be true without a direct experiment, is acknowledged; but taking into consideration the testimony that has been advanced, and the almost universal sentiment of the people who have experienced the disease, and have had it in their families, there appears to be much probability; and an opinion so general as this is among the people, ought not to be rejected without candid investigation.

An attack of the Sick Stomach is preceded by universal debility, more particularly felt in the lower extremities; impaired appetite, nausea, occasional vomiting, and an offensive breath, so peculiar as to be instantly recognized by any one conversant with the disease. In some, these symptoms constitute the whole of the disease, and exist for several weeks. In others they are followed, after some days or weeks continuance, by a more severe and general attack, characterized by great debility, sometimes irregular chills and flushings, a sense of great oppression about the precordia, anxiety, deep respiration, heat in the region of the stomach, compared by the patients to fire, boiling water, &c.; thirst, nausea, and at longer or shorter intervals, according to the

violence of the attack and to the period of the disease, violent retching and vomiting.

In the arterial system, the disease puts on a variety of aspects in different persons, and in different stages of the disease. In the same person, changes take place not unfrequently in the space of a few hours. The heart beats with such violence in some cases as to elevate the bed clothes, and to excite horror in the physician and by-standers on laying the hand upon the patient's breast. It seems to labour convulsively, and as though it were clogged in its motions by a superabundance of blood. The larger vessels partake of the heavy, throbbing and laborious motions of the heart. The blood forsakes the surface, especially that of the extremities, leaving it cold, and seems to rush to the larger internal vessels, engorging the viscera, and producing that oppression and anxiety about the precordia, sighing in respiration, palpitation of the heart, &c. The patient feels nothing which he can strictly denominate pain; but the sense of heat, the oppression, the palpitation of the heart, and the violent efforts to vomit, constitute an *extreme* degree of *distress.*

In other cases there is much less of this turmoil of the system. The patient, in the intervals of vomiting, lies in a stupid, listless condition, from which he is roused only by a returning fit of vomiting.

The state of the pulse varies with the other symptoms. In the early stage of the disease, it is sometimes full and tense, at others full and soft. Later in the disease it is slow and weak, exhibiting little or no febrile action, except it be of suffocated or at least unequal excitement. The bowels are obstinately costive, and the stomach so irritable, that medicines and drinks are rejected soon after they are swallowed. In the early period of the disease some bile is discharged, but never, I believe, in great quantities. The matter ejected afterwards seems to be little else than the drinks, &c. taken in, mixed perhaps with a secretion from the stomach; and in violent cases, a flaky substance varying in colour from a light brown to a deep black, is mixed with the fluids.

CASES.

ART. X. *Case of a singular Tumour within the Vena Cava and attached to the Eustachian valve.* Communicated by BENJ. H. COATES, M. D.

PROFESSOR Corvisart, in his work on diseases of the heart, mentions several cases of vegetations of the valves, and in one instance, of the internal surface of the heart—sometimes consisting of an alteration in the structure of the chordæ tendineæ, and sometimes resembling syphilitic warts. He conjectures them to be of that nature. He also mentions the ordinary polypi. Laennec mentions two species, of which one is similar in size to the warty excrescences just mentioned, and the other is of a form approaching to globular, near the size of an egg, and found in the cavities of the heart. These observations were, I believe, originally made by those two writers. It is well known that polypous concretions are liable to form in the blood vessels. In the following case the adhesion of such a body to what is called one of the valves of the heart, as well as its somewhat unusual situation, may render it not totally devoid of interest. I am indebted for many of the particulars to the copious notes of my friend Dr. Lawrance.

P. C., a native of the West Indies, aged twenty-six years, and of a middle stature, was admitted into the Pennsylvania Hospital, 8th month 13th, 1819, with a scirrhous testicle, combined with hydrocele, for which he had been tapped several times, but with small reduction of the tumour. The visible diseased mass was extirpated. The testicle was then about the size of an ostrich egg, and was divided in two parts by a tense band.

An enlargement soon after took place in the direction of the chord; which, from its situation could not be removed. The abdomen next swelled and reached a great size, with lancinating pains, and ultimately with constipation from pressure on the intestinal tube. The most remarkable feature in the history of the case, during the life of the patient, was the fact, that a mercurial course, which was tried among many other means, (the pains affecting his liver,) produced very great relief, and a considerable diminution of the swelling. He was almost flattered with the expectation of recovery, when he again grew worse; and on the 7th of the 4th month, 1820, all the appearances of internal scirrhus being present, he died.

On opening the body, an enormous tumour was found adhering to the peritoneum on the right side, and enveloping to at least one half their width, and with the firmest adhesions, the cœcum and parts of the colon and ileum, on the same side. A process of the same mass extended into the pelvis, included the rectum in the same manner, and adhered intimately to the urinary bladder. Another lobe of the tumour lay higher and deeper, and included the right kidney so completely that it could not, at first, be seen. A large fleshy process extended upwards, separated the duodenum several inches from the spine, adhered to the vena cava, embraced the whole circumference of the aorta, and finally became continuous with the diseased substance which formed nearly the whole of what appeared to be the liver. At the posterior part of the tumour was found the enlarged spermatic chord, extending to the place at which it had been tied, during life, and forming a blunt projection there, which had been felt externally. Its composition was similar to that of the rest of the tumour. The whole presented the mixture of cartilaginous bands with cells of softer matter, which occurs in scirrhus; occasionally with coagula of blood, and in the liver, with several small abscesses. The right kidney was very much enlarged from distention of the pelvis and infundibula, which were filled with urine. The internal and tubular portion

of this viscus was nearly removed. The ureter was of the usual size, but the passage of fluids through it was rendered difficult by sudden and acute turns. The other kidney was healthy. Much bloody fluid was found in the abdomen and the left side of the thorax. There is no memorandum of the right side.

The vena cava was distended to an enormous size, and nearly filled by the tumour which it is the object of this paper to describe. Some resistance was offered to the passage of a bougie, but it was our impression that the vein was pervious to blood. The mass in question measures, in the preparation before me, five and a half inches in length, and nine tenths of an inch in thickness, tapering gradually to both ends. It adhered, apparently, to the anterior side of the vena cava—but when torn away, left the surface smooth. It was afterwards apparent, that the connexion was by means of processes of the same nature, extending into a number of small veins, entering the cava, on that side, from the general external tumour. The two ends were firmly attached—the lower one to the bottom of a cavity with smooth edges, and which was probably the remains of an obliterated vein. The attachment was three sixteenths of an inch in thickness, flexible, and slightly wrinkled. It bore considerable handling without injury. The upper end of the polypus was unfortunately cut off from our having separated the heart, before we discovered the unnatural mass. It was attached to the edge of the Eustachian valve, about half an inch from the right extremity, by a neck of about one quarter of an inch in thickness, of a strong and dense texture, and with a surface resembling that of the living membrane of the auricle and vein. At a very short distance, however, it assumed the rough and fibrous appearance of the rest of the tumour. This body was apparently composed of fibrine, of a texture somewhat ragged, though unusually dense and firm. A probe passed from the section near the top, almost to the bottom, and with such ease as gave the idea of a narrow cavity. The whole appearance of this thick bridle was striking and singular.

Though it is not necessary to add many reflections to a case like this, it may not be improper to suggest the impression, that this polypus was rather the effect of the disease of the neighbouring parts affecting the blood, than that of mere quiescence.

ART. X. *Cases illustrative of the use of Emetics in Constipation of the bowels, with observations, &c. Communicated in a letter to John B. Beck, M. D.* By DAVID HOSACK, M. D., Professor of the Institutes and Practice of Medicine in the University of New York, and one of the Physicians of the New York Hospital.

New York, March 15th, 1822.

Dear Sir,

IN the treatment of constipation of the bowels, it appears to be generally agreed upon by the profession, that when we fail to evacuate the intestines by the various cathartic remedies ordinarily prescribed for this purpose, the last resource is mercury carried to the point of salivation; at the same time trusting to opiates and antispasmodics to allay the pain, and to the usual antiphlogistic means of subduing any inflammation of the bowels that may be induced in the course of the disease. In the year 1796, I communicated to Dr. George Pearson, of London, a remarkable instance of constipation yielding to this treatment, after twenty-one days' continuance of the disease.* Reflecting upon the manner in which mercury operates in relieving cases of this nature, after the gums have become affected, it occurred to me that the action of this metal is not only by its effects upon the secretions in general, but more especially upon the biliary organs. The dark coloured and acrid discharges that succeed to this operation

* See Duncan's Annals of Medicine for the year 1796, vol. i. Edinburgh.

of mercury, appear favourable to this explanation. In like manner the general derangement of the digestive organs, the foul tongue, the offensive breath, the sallow complexion, that usually precede and attend upon the first stage of this disease, no less point to this obstructed and torpid state of the liver, as having great agency at least in predisposing to the disease in question, while a check of the perspiration and certain articles of diet, are not unfrequently the exciting causes of irritation and of the spasm and inflammation which ordinarily constitute an attack of this complaint. With these views I was led to the use of emetics as best calculated to remove the hepatic obstruction which appears to lay the foundation of this disease, while by their febrifuge and antispasmodic operation they are no less useful in removing the fever, the inflammation and constriction, that constitute some of the most distressing as well as dangerous symptoms that attend a constipated state of the belly. The cases which have fallen under my care, since the adoption of this mode of treatment, are so favourable to the views suggested, that agreeably to my promise, I now submit them to your consideration, and if you deem them deserving more public notice, have the goodness to give them a place in your Journal.*

1st. Mr. W. B. of Dutchess county, who had suffered a very severe and dangerous attack of constipation, of which he had been relieved by mercury, prescribed by the late Dr. Bard, after a great variety of the ordinary cathartics had been administered in vain, when on a visit to New York, was seized with the same symptom of distress and obstruction of the bowels that appeared at the commencement of his first attack. I observed his countenance to be sallow, his tongue covered with a yellow sordes, his breath offensive, his stomach disturbed, but the arterial system not yet sensibly affected. The ordinary cathartic medicines being rejected, I gave him an emetic of tartarized antimony. It operated freely, producing a general relaxa-

* New York Medical and Physical Journal.

tion of the system, with some discharges by vomiting of bilious matter, which, from its viscidity and dark colour, appeared to have been long pent up in the gall bladder. They were succeeded by copious evacuations by stool. He was instantly relieved, and was himself sensible that by this timely prescription he had been saved from a renewal of the same sufferings he had experienced in his first attack.

2d. Mr. S. W. a merchant of this city, whom I had frequently attended in very severe and dangerous attacks of constipation, and which only yielded to mercury, administered to the point of salivation, some years since experienced a renewal of the symptoms that usually had announced a visitation of his disease. I found him in great pain, his bowels in a state of violent constriction, his pulse small, his countenance sallow and livid. I gave him instantly fifteen grains of ipecacuanha combined with two of emetic tartar. It operated freely, both by vomiting and by the bowels. He was immediately relieved, and had no return of the obstruction.

3d. Mr. W——e, a gentleman of the bar, who had also been frequently my patient under obstinate constipation, and had been relieved by blood-letting, blisters, the warm bath, enemata, and mercury, given to the extent of salivation, experienced one of his usual attacks. I gave him an emetic of ipecacuanha and tartarized antimony. It also had the same good effects of restoring the biliary evacuations, and of removing the intestinal obstructions, without having recourse to the ordinary efforts that had been directed on former occasions. I have in like manner relieved the same gentleman, by an emetic, in a second attack of a similar nature.

4th. The late lieutenant Walter Stewart, a highly meritorious officer of the United States' Navy, had suffered numerous attacks of this disease in Philadelphia. In some of those he had been relieved by blood-letting, the warm bath, and injections of tobacco smoke, as directed for him by the late Dr. Wistar, but in most the obstruction was not removed until recourse was had to mercury, adminis-

tered to the point of salivation, and which, in his case, required a much longer continuance of the medicine than is usually necessary to produce that effect. When in New York he twice experienced a return of the same disease; in both instances I directed for him an active emetic, which afforded him immediate relief, by producing copious bilious evacuations from the stomach, followed by large discharges from the bowels.

5th. Miss L———, who is constitutionally inclined to a torpid state of the intestines, and is very liable to obstinate attacks of constipation, has been three times under my care in this disease. After directing in vain the various cathartics ordinarily prescribed, and all the usual forms of enemata, I at length directed the emetic before mentioned: it was followed by immediate relief, although the disease had been of several days duration. Upon another occasion I directed the emetic in the commencement of her complaint, with the same successful result. The same young lady a short time since experienced a return of the same disease, but attended with more severe pain than she had suffered upon any preceding occasion. Being confined to my bed, I requested an eminent physician to visit her; he found her bowels had been confined about seven days, attended with symptoms of intestinal inflammation. After letting blood from the arm, he directed fomentations to be applied to the abdomen, and enemata variously composed to be administered, and advised castor oil, salts, calomel and jalap, in succession, but all to no purpose. The purgative medicines were rejected by vomiting; the enemata were returned as they were administered, without containing the least portion of fœcal matter. She earnestly solicited an emetic to be given her, urging that she had been relieved by this means in both her former attacks; this was declined by the physician, who considered it to be a hazardous practice to prescribe emetic medicines in a case of this nature. Recourse was had to calomel and opium; salivation was the result, but without the relief by the bowels which usually follows this operation of mercury. Upon

my recovery I visited her; she still suffered severe returns of pain: her mouth had been sore three or four days, but notwithstanding the use of enemata, which had been frequently administered, as fomentations to the bowels when her pain was most severe, no alvine discharge whatever had taken place. Although fifteen days had elapsed since the commencement of her obstruction, I gave her an active emetic, viz. fifteen grains of ipecacuanha and two of tartarized antimony, directing it to be repeated at the expiration of an hour if necessary; the first operated very freely, discharging a large quantity of dark-coloured bilious matter by vomiting, followed by similar copious evacuations by stool. In twenty-four hours she was so entirely relieved that my attendance became almost unnecessary, except for the removal of the soreness of the mouth that remained from the mercury that had been prescribed; for this purpose a gargle of yest, honey, and borax, was directed, with occasional small doses of calcined magnesia, to preserve the bowels in a soluble state.

6th. Captain A. who had contracted, in the East Indies, a disease of the liver, has recently been affected by a constipated state of the belly, attended with spontaneous vomiting; finding no relief from injections, and all cathartic means given by the mouth being rejected, I gave him an emetic. As in the former cases, it was followed by a general relaxation of the system, and plentiful discharges both by the stomach and bowels.

7th. In another case of a young gentleman, who had experienced many severe attacks of constipation, attended with high inflammatory symptoms, in which, in addition to the use of mercury, it had been necessary to make repeated use of the lancet and other means of counteracting the attending inflammation, I have also recently directed emetics with the best effects, not only in obtaining discharges from the stomach and bowels, but also in removing the pain, the spasmodic constriction, and febrile symptoms, which have usually attended every return of his disease.

From these facts I have been led to the following conclusions.

1st. That a constipation of the bowels is usually attended with, and frequently produced by, a torpid state of the liver, and consequent deficiency of the biliary discharge.

2d. That the pain, spasmodic constriction, and inflammation, attendant upon this disease, are the result either of the mechanical obstruction occasioned by the deficiency of bile, and consequently a retarded peristaltic movement of the intestines, or the effect of a sudden change of perspiration, or of a particular article of diet.

3d. That in the commencement of constipation, or in its more advanced stage, when the symptoms of inflammation have been subdued by the lancet, emetics may be very advantageously exhibited both for the purpose of removing the hepatic obstruction and of counteracting the spasmodic constriction and pain ordinarily attendant upon this disease.

4th. That the salutary effects which have been occasionally derived from injections of tobacco smoke, are attributable to the general relaxation, the nausea, and in some cases the vomiting, which that narcotic produces.

5th. That the benefits that have in like manner been obtained in some cases from the use of tartarized antimony, administered by injection, are to be accounted for by the nausea and vomiting that have been the effects of its operation, but which are to be obtained with more certainty from the same medicine given by the stomach, and to the extent of full vomiting.

In like manner, inasmuch as a torpid state of the liver and a diminished secretion of bile are generally known to constitute a part of the proximate cause of dysentery,* we obtain a satisfactory solution of the salutary operation of injections of ipecacuanha in that disease, as advised by Dr. Thomas Clark;† but which effects are more certainly to

* See Dr. Monro, Cleghorn, Pringle, and Girdlestone.

† Observations on the diseases of the East and West Indies, by Thomas Clark.

be obtained from the use of ipecacuanha given by the stomach, as prescribed by Sir John Pringle, or of tartarized antimony, as recommended by Senac.

With the hope that these facts, and the principles deduced from them, may lead to a more satisfactory view of the pathology and treatment of the disease which is the subject of this communication,

I am, Dear Sir,

Respectfully and truly your's,

DAVID HOSACK.

Dr. John B. Beck.

REVIEWS.

ART. XI.—*An Inaugural Dissertation on the Pathology of the Human Fluids.* By JACOB DYCKMAN, A. B., member of the Medical and Physical Society of the University of New York. 248 pp. 8vo.*

THIS is a production somewhat antiquated in more than one sense of the term. The views which it exhibits, are certainly not modern, and the date of its publication is of several years. Yet it has claims, on other grounds than its novelty, to attention, which will presently appear.

The medical men in the United States, as elsewhere, may be arranged under the two great leading classes of *humoralists* and *solidists*. Of the former sect, by far the most conspicuous, in every respect, is the learned professor of the theory and practice of medicine in the school of New York, by whom this in some degree obsolete creed, has been revived—and in an amended shape, is now sustained with his wonted zeal and ability.

* With the exception of one or two of the initial and concluding paragraphs, we are indebted for this Review to a writer of no ordinary authority. He has taken a very comprehensive view of his subject, and treated it with much force and ingenuity. We remark, that the New York Medical and Physical Journal is a good deal occupied with this discussion, and that we, particularly, are considered as the great offender. The liberality with which the controversy is conducted, calls for an answer, which shall be given in our next number, in the same spirit of courtesy and good breeding.—EDITOR.

The essay before us, purports to be the Inaugural Thesis of a private pupil, who, imbibing in the utmost extent the doctrines of the preceptor, thus comes forward with them, fully exposed, and most elaborately defended. There is abundant evidence in the constant reference to the public lectures and private communications of the professor, of a strict conformity to his tenets, and indeed the essay has always been regarded among us, as a sort of indirect or informal manifesto of the system of opinions of the school from which it issues.

We are of the other side—*solidists*, from conviction, and feel naturally a desire which even our well known aversion to polemics cannot resist, to be heard in the case. The subject is deeply interesting in all its relations, and we avail ourselves of this, the earliest and most favourable occasion, to enter into a candid and amicable discussion of it.

Besides a number of insulated and minor positions, which we shall not be permitted to notice, this essay clearly enunciates and endeavours to maintain, five leading points in humoral pathology.

I. That most, if not all diseases, are *primarily* radicated in the fluids. This constitutes essentially the basis of the doctrine.

II. That the blood is often marked by a *morbific lentor*.*

III. That this fluid contains also, at times, sundry kinds of morbific acrimony received *from without*.†

IV. That it contains likewise a *putrid* acrimony *engendered within*.‡

V. That, in contagious diseases, it is the medium in which the morbific poison is generated, or multiplied, and the vehicle by which it is diffused throughout the system.§ These several positions we shall proceed to examine.

* Vide p. 78, to p. 79.
† Vide p. 95.
‡ Vide p. 85, to p. 86. Again, p. 145.
§ Vide p. 186.

I. Which of the two, the solids or the fluids, are, in the embryotic state of man, antecedent in existence, is, as far as relates to the present question, a matter of no moment. After the body has become sufficiently organized to be a subject of satisfactory observation, *the fluids are unquestionably the product of the solids.* In all their qualities, they are as literally the result of organic action, as any manufactured article is of the operation of the artificer. They are not the offspring of chemical agency. If they were, chemistry would be able to form or closely imitate some one of them *out of the body.* But this is not the case. In all the labours and multiplied processes of his laboratory, no chemist has ever yet manufactured the simplest of the animal fluids. Nor is he justified in asserting the practicability of such an event. Chemistry being an experimental and demonstrative branch of science, its limits, at any given period, are accurately defined, and well understood. But what may be its limits at a subsequent period, must depend entirely on improvements and discoveries yet to be made, and cannot, therefore, be certainly predicted.

In the present state of the science, nothing but ignorance or presumption can induce physiologists to attribute to chemistry any share in the formation of our fluids. To say, as some have done, that these fluids are produced partly by chemical agency, and in part by organic action, is an error, if possible more gross and offensive, than that which derives them exclusively from the former source. Every fact and consideration bearing on the subject, unite to persuade us, that within the sphere of vital agency, the chemical affinities cease to be operative. Not until organic action is destroyed can chemical action begin. Light and darkness, heat and cold, virtue and vice, are not more essentially opposed to, and inconsistent with each other, than the powers and processes of chemistry and vitality.

Our fluids, then, we repeat, are the product of our solids. The latter are the cause, the former the effect. But, while the cause is sound, it is contrary to nature that the effect should be morbid.

While our solids are healthy in structure and in formation, so will be, so *must* be, the fluids that proceed from their action. To this rule there can be no exception, or else there is an end to the necessary and eternal relation between cause and effect, and all nature becomes a chaos and a riddle. Were it permitted us to dwell on it, we could reduce this position to the precision and certainty of mathematical demonstration.

From the operation of *interior* causes, then, it is impossible for the fluids to be *primarily* diseased. Nor, in relation to the action of causes *from without*, does the position appear to be less sustainable. In a natural and healthy state of things, our solids every where inclose our fluids, as a wall of safety from external injury. Before a noxious agent can act on the latter, it must necessarily make its way through the former. But, in effecting its passage, how can it avoid doing more or less mischief? how avoid deranging the structure, or the functions, or both, of the solid parts which it touches in its course? If a poison, or any other deleterious substance can pass into the blood, commingle with that fluid, and then injure the solids, it *can* unquestionably, it *must*, produce a similar injury during its passage through the solids. It is even more likely to do mischief during its passage than afterwards, because it is to be regarded as then in a more concentrated state. Every one will acknowledge that no mechanical substance can penetrate into the blood vessels without injury to the solids, because the wound inflicted is palpable to sight. Nor, to the eye of reason, if skilfully applied, is the mischief done by a poisonous substance similarly situated, less palpable. Were we as much the creatures of reason as we are of sense, as much accustomed to use our intellect as our eyes, this assertion would cease to appear doubtful.

As soon would we believe that a stiletto can penetrate into the blood of an artery or a vein, without wounding any of the surrounding solids, as that a substance really poisonous can pass innoxiously along the lacteals or lymphatics.

Were evidence corroborative of our opinion wanting, it might be abundantly derived from the phenomena arising from the introduction into the skin, by means of a puncture, of the variolous, venereal, or vaccine virus, or any purulent deleterious fluid. By such inoculation the solids are always *primarily* diseased.

From these considerations, to which the limits of this review will not permit us to subjoin sundry others that spontaneously present themselves, we feel persuaded that neither from within nor without, does disease ever *primarily* attack the fluids. It always and necessarily commences in the solids, by the morbid action of which only can the fluids become deranged.

Even when noxious articles are intentionally introduced into the vascular system, it is not the fluids, but the solids that suffer. This assertion we make, not on the ground of conjecture, but of fact—having very often injected deleterious substances into the blood vessels of living animals. In these experiments, exquisite pain, tremors, vomiting, purging, convulsions, and death, were almost always suddenly produced—while, in its sensible qualities, the blood sustained no perceptible change—or, if any perceptible change did occur, it was in its colour, which was usually, perhaps, rendered preternaturally dark. Another effect of these fatal injections was, that the blood rarely, if ever, coagulated, its vitality being completely destroyed by the poison introduced.

II. Whether, in any case, the blood is ever marked by a morbific lentor, or preternatural viscidity, is a question to be settled by observation only. On this topic, neither reading nor closet speculations can avail. Yet books constitute, as we believe, the only sources from which our author has received his information. On this point, it must be confessed that his book knowledge is considerable—though we are concerned to add, that it is as fruitless as it is ample. Never has he been more unprofitably employed, than when ransacking the tomes of former times, in quest of evidence

to prove the existence of a *lentor* in the blood. We beg leave to assure him, that, touching this subject, there exists a book, pregnant with information much more correct and valuable, than any to be derived from the writings of Browne Langrish, Van Swieten, or Boerhaave. Nor is it necessary that we should either travel far to gain access to it, import it from abroad at a heavy expense, or laboriously search for it in the libraries of the curious. It is every where to be found, written in a language universally understood, and opens to all its inexhaustible treasures. We allude to the book of Nature—by a steady and faithful perusal of which, he could not fail to be instructed. To the pages of this volume we respectfully refer him.

Have not our cotemporaries blood in their veins, as well as the people of former ages? Have we not eyes and other senses as keen and as faithful as our predecessors in Europe? And are we not permitted and virtually commanded, to employ those prerogatives which God has bestowed on us, as the means of our instruction?

Whence is it, then, that we humbly and servilely depend on *man*, for that knowledge which *Nature* herself is tendering to us, and which it should be our pride to receive at her shrine, in its original freshness and purity? More disgraceful, still,—wherefore is it that we prefer the dreams, and visions, and fallacies of the former, to the solid and everlasting truths of the latter?

Is there never to be an end of this colonial humiliating looking up to the *mother country* for all the intellectual riches we possess? Shall the people of Europe be permitted and even courted eternally to manufacture for us the thoughts we are to express, as well as the garments we are to wear? Shall we call ourselves free, and yet ignominiously submit to their professional dictations, and tamely allow ourselves to be surfeited with their sickening dogmas, as if we were still the lowliest of their provincials?—That there are *some Americans* who will thus cringe and truckle

to the *scavans* of Europe, we blush to acknowledge.* But we feel proud in the confidence, that the number is small, and that a national spirit is rapidly taking possession of us, which will induce us to turn indignantly from the adulterous *common-place* of book-makers abroad, to the pure and choice and never-failing resources of nature at home.

Let us not be regarded as an enemy to reading. Far from it! To be cultivated and enlightened, we must read extensively. But while consulting books, let no votary of truth and genuine knowledge fail to *doubt*, *select*, *observe*, *experiment*, *reflect*, and *compose*—a process of intellection to which too many are strangers. To read *only*, and believe every thing, is to discipline in error.

Before he ventures to write on the subject again, we entreat our author to examine attentively and without prepossession, the blood of man, in health and in disease. He will then be convinced of his mistakes, and of the degree in which he has suffered himself to be led astray, by the meteor of authority. In relation to the point in discussion, we profess to have some knowledge, drawn from the best and most authentic of sources, observation and experiment. We trust that he will not take offence at us for faithfully, but very briefly attempting to impart to him a small portion of it.

We would first inform him, that the blood, being a *living fluid*, (a truth which he admits) we can never infer its state and character while circulating in its vessels, from the appearances it exhibits soon after it has been drawn. In the former case it is a fluid perfectly homogeneous, its elementary parts so harmoniously combined, as to constitute a unit. It does not consist of serum, red globules, and fibrin, each in a formal and insulated state, nor yet in a state of *mechanical or chemical admixture*. By the agency of the *vital principle*, these three articles are so intimately and specifically united, as to be literally *tria juncta in uno*, a

* We beg to be understood as not including our author among such Americans. We are too well assured of his manly and erect spirit, to ascribe any such disposition to him.

tri-une combination, and to constitute a fluid possessing properties, and manifesting phenomena, peculiar to itself.

In the latter case, after it has been drawn, the situation and state of the blood are entirely reversed. It is now in contact with dead matter, and in consequence of the deleterious impressions which it thence experiences, is undergoing changes that amount to a decomposition. It is *dying*, and, in its last struggles (for such they really are) its component parts, having been but feebly bound together, separate for ever. It is now that the coagulating lymph, beginning to contract, assumes a viscidity,—*the Boerhaavian lentor*,—which it is mistakenly supposed to have possessed while flowing in the vascular system. Far from being true that the *viscidity* of the blood is *increased* during any stage of inflammatory and other fevers, it is actually *diminished*. In pleurisy, peripneumony, pulmonary consumption, hepatitis, and rheumatism, that fluid is perceptibly thinner than it is in a state of perfect health. Nor, notwithstanding the assertion of our author to the contrary, does it manifest the slightest degree of preternatural viscidity, in what he denominates leucophlegmatic affections. Respecting all other maladies, in which he declares it to exist, we deliberately affirm, on the ground of observation, that the same thing is true. In no one of them can the slightest viscidity of the blood be found, as it flows fresh from the vein. But when suffered to remain for a time at rest, subject to the irritating impressions of the atmosphere and the vessel that contains it, it never fails to exhibit such viscidity. *The blood of health, however, manifests it sooner than that of fever*—a phenomenon easily explained by those who have made themselves acquainted with the vital properties of that fluid.

An idler dream than this lentor of the blood, has never disturbed the slumbers of any visionary. It is one of the coarsest and grossest superstitions in medicine, the fallacy of which may be easily detected, and satisfactorily exposed. The only thing requisite is, to open a vein, and carefully examine the blood as it flows. If our senses have not deceived us a thousand times, the fabled lentor of that

fluid will vanish before this simple process, like vapour under the influence of the mid-day sun.

III. That certain matters which may be denominated *acrimonious* have been found in the blood, when *dead*, tortured by various transmuting processes, and subjected to tests in the chemist's laboratory, is, no doubt, true. But does it thence follow, that those articles, in a formal and insulated state, existed in that fluid when circulating, in a homogeneous and vital condition, through the arteries and veins? Whatever may be the opinion of chemical philosophers, without evidence of the fact much more conclusive than any heretofore adduced, no physiologist will reply in the affirmative.

On this and many other points of no ordinary weight and interest, chemistry has adulterated and darkened physiology. In his experiments on the blood, as well as in those on sundry other substances, the chemist, by his decompositions and recompositions, and all his varied range of mutations, actually *makes* what he fancies he *discovers already existing*.

He buries in the earth, exposes to the action of nitrous acid, or suspends in a stream of gently running water, a piece of *dead* muscle, and obtains *adipocine*. But he must not hence contend that that substance existed *formally* in the *living* muscle. He burns a hickory log, and procures from the ashes a portion of *potash*. But he shall not persuade us that this substance had a *formal* existence in the living hickory tree as it flourished in the forest.

To the putrefactive process he exposes an onion, and obtains phosphorated hydrogen gas—a piece of flesh, and the result is a gas sometimes phosphorated, and at other times sulphurated. But to assert that real phosphorus exists in a living onion, or, either that substance or sulphur, in the flesh of a living animal, would be preposterous.

In like manner, when he finds in blood, dead and decomposed, various ingredients of an acrimonious nature, he has no authority to infer that they distinctly existed in that fluid when living, homogeneous, and flowing through

its vessels. We repeat, that the substances which he reports as component parts of the articles he has been analyzing, are nothing save the offspring of his own experiments. Nature has only furnished the materials to him who is the manufacturer. *By organic action*, she has formed muscle, and blood, and wood, and onion, which are *vital compounds*, and he, by decomposing or recomposing her productions, gives origin to such substances, simple or compound, as *belong to chemistry*.

If acrimonious matters ever enter the blood from without, they must effect their passage through the channel either of the lacteals, or that branch of the absorbents which is connected with the skin. But even admitting, so far as *mild articles* are concerned, the *very doubtful* doctrine of cutaneous absorption, it is totally *inadmissible* in relation to *acrid ones*—at least, such a process can never take place in an imperceptible manner—in proof of which, we have abundant testimony.

If a small quantity of variolous poison be laid on the skin, where the cuticle is sound, it produces no effect. The part itself does not inflame, no injury is sustained by any lymphatic vessel or gland, nor does a constitutional affection ensue. In common medical phraseology, the contagion does not enter the system. The same thing is true in relation to every other kind of animal poison, native or morbid, with which we are acquainted. Not one of them can make its way into the system, provided it be only applied to the unbroken cuticle. To this neither the poison of the rattle-snake, the venereal virus, nor the more formidable matter of rabies canina, constitutes an exception.

There is the strongest ground not only of belief, but of actual conviction, that the sound cuticle serves also as a panoply against every kind of mineral and aerial poison. It has been long known, that where that covering is entire, a bath containing in solution corrosive sublimate, is perfectly inoperative. So are all the vegetable poisons with which we have experimented, except those that stimulate so powerfully as to produce vesication, none of which have ever

been believed to make their way into the blood through the cutaneous absorbents. Generally, then, no poisons of any description can pass into the system, by way of the skin, unless that organ be scratched or denuded. But if the cuticle be abraded, or the cutis vera punctured, the case is different. Then, *according to common belief*, absorption by the cutaneous lymphatics is effected—though never imperceptibly to the person who suffers. The progress of the poison or acrimonious article, is obvious alike to feeling and to sight.

By the puncture of a lancet, a portion of variolous or vaccine matter is introduced into the skin. The uniform consequences are, a local inflammation, an enlargement and soreness of a lymphatic gland through which the absorbent vessels of the part pass, accompanied very often by a tenderness of the vessels themselves, and lastly, a general constitutional affection. Introduce in the same way a portion of the matter of gangrene, or of any other deleterious animal acrimony, and a train of analogous phenomena will ensue. The effects of the application of chancrous matter to the glans penis, or the mucous lining of the prepuce, are too well known to require a recital. From these considerations we hold it perfectly obvious, that, through the cutaneous absorbents, no acrimonious substances can *unperceived* make their way into the system. If they could, the condition of man would be much more precarious and lamentable than it is. The crime of poisoning would be easily perpetrated, and myriads of avenues would be thrown open, through which the arrows of accidental fate might imperceptibly pass. Under such an arrangement, the human race must cease to exist. But no one has ever yet alleged that he was *sensible* of the passage of the variolous poison, when casually received, or of any other acrimony into his blood.

We ourselves do not believe that either the virus of small pox, of kine pox, or of lues venerea, or any other acrimonious substance, *even when inserted into the skin by the puncture of a lancet*, is ever taken up by the cutaneous absorbents. Even here, propitious to such an event as the

condition of things is supposed to be, no contamination of the fluids ensues. We wish this to be understood as our deliberate opinion. We cherish a perfect and long-existing conviction, that all the effects which follow the operation are exclusively the result of primary local irritation and subsequent sympathy. In the way of causation, the fluids have no participation in the business. Nor have the lymphatics or their glands, except in the capacity of irritable and sympathizing solids. On this subject we shall have occasion to speak more fully hereafter.

But for the sake of argument, that the topic may be examined in its most prominent bearings, we will accede, for a moment, to the opposite notion. We will admit the practicability of acrimonious substances being taken up by the cutaneous absorbents. Even then, they could not *as acrimonies* pass along them, and find their way into the blood. By the *assimilating agency* of the lymphatic vessels and glands, they would be deprived of their deleterious qualities, and converted into lymph. The existence of this agency is no fancy. It is, on the contrary, as well established as any point in physiology.

The lymph, though formed out of bone, cartilage, tendon, muscle, nerve, and every other kind of substance that enters into the composition of the human body, is notwithstanding a *homogeneous fluid.* It does not contain, *in a formal condition*, the component parts of any of the organs from which it is derived—a state of things ascribable exclusively to the digestive or assimilating power of the lymphatics and their glands. But if these organs can so far effect a change in gelatine, fibrin, and phosphate of lime, as to convert them into a limpid and homogeneous fluid, there seems no reason to doubt their power to take from acrid substances their deleterious qualities. Besides, as such substances have never yet been found in the cutaneous absorbents, no one can contend for their existence there, consistently with the rules of inductive philosophy. But should they, by some means, gain admission, and resist the efforts of the lymphatics to change them, they would be

arrested in their course either by the vessels themselves, or in one of the glands with which they are connected. Should they, by any possible chance, surmount all these difficulties, and make their way into the blood, even there, the vital and self-preservative power of the vessels would change and assimilate them.

To the belief that acrimonious matters can pass into the blood through the medium of the lacteals, a series of arguments somewhat similar may be still more clearly and powerfully opposed. Before reaching the mouths of those vessels, such articles must pass through the stomach, of the transmuting power of which no one can be ignorant. They must also sustain, in part, the action of the duodenum. The lacteals themselves are capable of an assimilating agency, and are known to exert it.

To the admission of acrimonies into the blood, then, through this channel, are opposed the action and energies of three distinct and powerful organs, each containing a specific capability. Nor is this all. Such appears to be the peculiar appetency of the lacteals at their mouths, that they will admit none except congenial substances. The uncongenial and offensive they promptly reject. As far as we are informed, such is the result of the most conclusive experiments that have been made on this subject. It is certainly the result of all that we have ourselves performed.

Every practicable avenue, therefore, being insuperably barred, we deliberately deny the admission from without of all deleterious acrimonies into the blood. The belief in the admission of vegetable acrimonies, is indeed a dream which does not deserve refutation. Yet, considerations not to be disregarded, invite us to notice it.

If garlic, onions, assafœtida, spirit of turpentine, or asparagus, be received into the stomach, their smell, or taste, or both, will, in a short time, be perceptible in the urine and perspiration, and also in the milk of lactescent females. Now, say our antagonists, in as much as these secreted fluids are formed from the blood, those vegetable acrimonies (for so the matters of taste and odour may be denomi-

nated) being *formally* present in such fluids, must be, in like manner, present in the blood from which they are derived. To this intended syllogism we reply, that, in the present instance, our business is, not with a process of reasoning, though apparently plausible, but with a well established fact: and, on the basis of repeated experiments, we assert it to be a fact, that when the taste and smell of the preceding articles are very strongly perceptible in the secreted fluids specified, *they are not in the slightest degree perceptible in the blood.* Were the case otherwise, the blood being no longer homogeneous, would cease to be blood. In inferior animals fed on garlic, the chyle also is free from the odour of that article. We shall only add, that in the blood of jaundiced patients, though after its death and a separation of its parts, the serum is yellow, there is none of the acrimony or the bitterness of bile.

Experiments on this subject are easily made. Let them be pursued and reiterated, and on the result of such of them as are judiciously conducted, we are willing to rest, without the least apprehension, the issue of the controversy. The fact, in itself, is not a little curious, nor is its explanation easy. Do the component corpuscles of the sapid and odorous portions of the vegetables, having been separated, subdued, and assimilated while in the blood, reunite by chemical attraction, so soon as they are thrown as a part of the secreted fluids, without the sphere of vital organic action? or do those stimulating substances, like the matter of small pox, irritate the stomach in a way so peculiar, as to produce their own likeness, through the medium of sympathy, in some of the secretory organs of the body? In language more definite, does the matter constituting the odour and taste of garlic, when taken into the stomach, so stimulate that organ, as to excrete, by sympathy, in the glands of the female mammæ, a specific and corresponding action, by means of which a matter marked by a similar taste and odour is secreted from the blood, though possessed of its usual purity? However visionary, at first view, this hypothesis may appear, considerations are not wanting to render

it plausible. But the fact, and not the philosophy, constitutes the object of our present research.

IV. The error we have to combat under this head is, if possible, more glaring, than either of those we have heretofore examined.

A *putrescent* means, in plain English, a *rotting* state of the blood, which is again synonymous with that state in which a degree of *putridity* already exists. To Professor Hosack's "*incipient putrefaction*," of that fluid we must attach the same meaning, if, indeed, we concede to the expression any definite meaning at all. Torture and modify definitions as our author may, we are supported in the interpretation we have here given, by the ablest lexicographers in the English language.

To affect to make a distinction between an *incipient* or *beginning* state of things, and a state already *in actual existence*, is idle. As soon as a thing has *begun to be*, *it actually is*—as for example—It is *beginning to dawn*, in other words, *it dawns*—It *begins to rain*, it *is raining*—Clouds begin to appear, clouds *have* appeared—The wind *begins to blow*, the wind *is blowing*—This mass *begins to putrefy*, it *is* putrefying, it is *somewhat putrid*—It *begins to move*, it *is moving*. These are synonymous expressions, the mere duplicates of each other, differing only in the amount or intensity of the state, quality, or attribute, which they indicate.

On a fair construction of language, then, both our author and his preceptor declare, that a condition of the blood in which putrefaction has *actually commenced*, is compatible with the existence of animal life. An error of much greater ormity than this has never in our opinion appeared on the page of medicine, and it is the more so, in consideration of the time in which it is promulgated. Those physicians, many of whom were pre-eminent in their day, and whose writings on various professional topics are still among the most valuable beacon-lights of the world—those physicians, we say, who believed in this doctrine a century or two ago, were comparatively excusable. The adulterated philoso-

phy of the schools, could not fail to stamp its impress on their intellect and their knowledge, and the error they adopted was their misfortune rather than their fault. No philosopher can insulate himself from the age in which he lives, nor leave it so far behind him in his march, as not to be materially modified by its influence.

Had it been the fortune of those distinguished men, from whose works our author and his preceptor have derived their opinions, to live at the present day, far different would have been their views of things from those they entertained at the time they wrote. Discarding the dreams and corruptions of *humoralism*, they would have been leaders and teachers in the sect of solidists. Could the great Boerhaave or his pupil Van Swieten be recalled from the tomb, they would reject with disdain their *lentor* and their *acrimony*, as untenable amid the splendid and pervading illumination of the nineteenth century!

We have already had occasion to lament, that, to the neglect of the study of nature, our author derives his notions too exclusively from books. Even here again we are reluctantly compelled to enter our protest against his choice of authorities. Though ourselves enthusiastic admirers of many of the productions of antiquity and of the dark and middle ages, and not a little devoted to the study of doctrines and opinions, the intellectual growth of distant countries—we are so unfashionable as to say, that, neither in literature, science, nor the arts, does mere distance of time or place consecrate in our view the labours of man. Nature is the great touchstone, by which we wish to see all that is human rigidly tested, and as far as our individual exertions can avail, we endeavour to contribute to the important work. Whether ancient or modern, and whether sanctioned by great names or by no name at all, we hold it degrading to receive as authority any opinion, without subjecting it to a deliberate scrutiny.

If we be inclined to view with marked indulgence and partiality any of the productions of the human intellect, we acknowledge it to be those of our own country. What-

ever is American, if it possess any tolerable share of merit, appeals to us with a force which we find it not only difficult, but painful to resist. We shall here take the liberty of declaring, however ungraciously the sentiment may be received by the votaries of Europe, and of "times long gone by," that much instruction may be derived by a careful perusal of certain American productions of modern date. This is very particularly the case, in relation to the notion of putridity in the blood.

If we are not greatly mistaken, Dr. Seybert, of this city, in his Inaugural Dissertation, published in the year 1793, has shed more genuine light on that subject, than Browne Langrish, and Boerhaave, and Huxham, and Pringle and Lind, and all other writers on Medicine that Europe had antecedently or has subsequently produced. As every philosopher ought to do, in such a case, and as every true one will do, he took the business entirely out of the hand of man, and applied for information at the shrine of nature.

His Dissertation is experimental. The various topics in it are judiciously selected and ably handled, and the whole doctrine of sanguineous putridity triumphantly refuted. We regret exceedingly that the narrow limits within which we are restricted forbid us to enrich our pages with copious extracts from it.*

We shall however offer to our readers no apology for inviting their attention to the following inferences, fairly and legitimately deduced by Dr. Seybert, from a very laborious and excellent series of experiments, which he had previously performed.

"1st. That, contrary to the general received opinion, the blood is neither rendered alkaline, acrid nor putrid by starvation. This I think is evinced by these circumstances—that the blood went through its spontaneous changes in the regular and usual manner—that the serum had no effect in changing the colour of my test—and that no sense of acrimony was perceptible to the taste.

* The Dissertation is contained in the volume of select "Medical Theses," for the year 1805, published by Thomas & William Bradford.

"2dly. That a putrid diet does not operate upon the blood so as to change its sensible properties. This is a material part of the inquiry, since physicians, from Hippocrates down to the present day, have supposed various and wonderful changes to be produced in the animal economy by such food. From the above experiments, it appears very clear, that a putrid diet had no effect whatever in changing the qualities of the blood, although the animals were strictly confined to it. We are also led to conclude from them, that a putrid diet does not change the sensible qualities of the excretions; since, as I have related, the urine did not alter the colour of the vegetable blue. Here it may be objected to me, that I ought merely to infer from these experiments, that the blood of the dog only is not affected by this treatment. I answer, that the inference may, probably, be extended to the whole animal creation; for, we cannot but suppose that nature is, in this respect, as beneficent to the one species as she is to the other; and to substantiate this opinion, I avail myself of the result of the experiments of the celebrated Spallanzani, as above mentioned, who not only operated upon the brute creation, but went so far as to risk his own life for the benefit of science; and found, contrary to the received opinion, that vitiated food, previous to its leaving the stomach, is converted into a matter capable of furnishing good blood. 'Men, such as the inhabitants about the mouth of the Orange river, in Africa, live always on animal food, such as whales, seals, limpets, and what fish they can catch; that many times their food has entered into a great degree of putrefaction, and there is no vegetable food whatever employed at the same time; probably most of them never tasted any vegetable substance in their lives, excepting aromatics for seasoning; yet they are perfectly healthy and free from all putrefaction in their fluids or solids, though they are not very careful of avoiding it in the exterior parts of the body. We see likewise maggots live in and upon putrid masses, while they themselves, and all their fluids, are perfectly sweet and free from all appearance of putre-

faction.'* There appears to be an intimate connexion between the fluid and solid parts of our bodies; but how far this connexion between them subsists, I will not undertake to assert. Perhaps I might, in truth, have said that a putrid diet acts upon the solids of our machine, by its not containing alimentary matter in a proper quantity, and therefore not affording a sufficiency of blood to support the animal. We know that the solids derive their nourishment from the blood; and, in my opinion, it is sufficiently evidenced, from the mortification of the muscular parts which ensued upon making an incision into the thigh, as has been already stated, that a putrid diet does operate upon the solids.

"3dly. That although the blood, or other animal matter, may be excited into a putrid fermentation out of the body, by the addition of a putrid ferment, yet that such a process cannot be excited in the living body. We have found that many grains of putrid matter existed in the blood-vessels for some days without changing the blood. Is it probable that a fermentation can be induced in the blood when the ferment enters the system by absorption, and no such process take place by introducing a putrid ferment immediately into the blood vessels?

"4thly. That by the introduction of putrid matter into the blood vessels, very violent symptoms were produced, although the quantity of the matter was small, and of the same nature as the blood, except that it had become putrid.† Is it probable, then, that the animal could exist with the whole mass of blood in a state of putrefaction, when such violent symptoms were the consequence of the introduction of so small a quantity of putrid matter as was injected in the foregoing experiments?

* Fordyce on Digestion, p. 155.

† My reason for making use of putrid serum and putrid blood, is, that they are the same substance, only altered by putrefaction. We know, from experiments of transfusion, that blood may be passed from the vessels of one animal, into those of another, without any evident injury. From this circumstance we are led to infer, that it, in these instances, acted as putrid matter.

"And, 5thly. That though the volatile alkali may operate upon the blood in a wonderful manner out of the body, it does not evidently affect the blood when injected into the blood vessels.

"From these experiments, it is also evident, that cathartic and emetic medicines, when injected into the blood-vessels, cannot operate in a specific manner; for in almost every instance evacuations were the consequence of the experiments, when neither cathartics nor emetics were injected. Probably any matter capable of producing a sufficient irritation, will produce those effects, when thus thrown into the animal."

So ample and conclusive are these inductions, and so authentic the premises from which they are drawn, as to preclude the necessity of our dwelling on the subject.

We have repeated ourselves, or seen repeated by others, most of the experiments of Dr. Seybert, and know the report he has given of them to be correct. We further know, and to every man of observation and intelligence, the truth presents itself with a force which is irresistible, that through all nature there is an irreconcileable hostility between *putrefaction* and *vitality*. The very import of the terms establishes our position. Not greater or more striking is the incompatibility between *life* and *death*. It is indeed the same, or, rather, that between life and putrefaction is greatest, in as much as death may be said to occupy an intervening station. The body, whether animal or vegetable, dies first and putrefies afterwards. Never has man seen this order of things reversed. Nor is it possible for it to be reversed, until the present system of nature undergo a change.

Looking abroad through the whole range of organized matter, we shall perceive between vitality and the process of putrefaction, a warfare that is universal, eternal, and exterminatory. No sooner does the vital principle forsake a body which it has long animated and held together, than putrefaction commences the work of demolition. It is the province of the former to build up and preserve,

of the latter to resolve into primitive elements. Never can they both at once exist in the same volume of matter. This is the declaration of nature herself, re-echoed by every organized being. As soon can love and hatred, piety and impiety, or any other opposites, coexist as motives to the same actions.

The vital principle is the most potent of antiseptics. Its common and universal office is to resist putrefaction. Hence, *living* seeds, and animals *living*, but in a state of torpidity, have remained fresh and entire for many centuries. Extinguish in them the vital spark, and in a few days they are elementary matter. When life forsakes them, and not until then, putrefaction makes them its prey.

When putrid aliment is received into a sound and vigorous stomach, the vital powers of that organ remove the taint, and restore to the mass its original sweetness. To every physiologist sufficiently versed in the knowledge of digestion, this fact is perfectly familiar. It may be abundantly verified by an examination of the contents of the stomachs of vultures, ravens, dogs, and the whole tribe of beings that subsist on carrion. However putrid and offensive the mass may have been previously, it is found, soon after having been swallowed, to emit no other odour than that which is common to the stomachs of carnivorous animals, and which, though not pleasant, bears no resemblance to the odour of putrefaction.

Nor is this all. Though the exhalations of vultures and other animals that feed on carrion, are in no small degree offensive, their blood is as free from taint, as that of beings subsisting on the daintiest fare. Inject into their blood vessels putrid articles, and we destroy them as readily as if they fed on the freshest vegetables. To these experiments, in a particular manner, we earnestly invite the attention of our readers. They are all-important in the present discussion. They prove conclusively, that a mixture of putrid matter in the blood of animals, even of those to which it might well be expected to do least harm, is incompatible with life. What, then, must be its effects

in the blood of man, whose susceptibilities are so much more tender and acute?

We are told by our author, that, in a given instance, the human blood, when recently drawn, was found to emit a putrid odour. So contrary is this allegation to all we have seen or known, and so directly opposed to what we believe to be an established law of nature, that, without meaning to impeach the veracity of any one, we unhesitatingly disbelieve it. As soon would we credit the declaration, that maggots, fleas or frogs had been discovered in the blood as it circulates through its vessels.

V. Under this head of our subject we shall only so far refer to the doctrine of contagion in general, as to say, that in the whole range of medical science there is no topic that has been treated in a manner more loose and inconclusive. A thorough, sound and lucid philosophical investigation, it has never received. Much has indeed been written, and much done in relation to it. Its ravages have been described in language to make the benevolent weep and the fearful tremble. Buildings have been erected to incarcerate it with their walls—fumes and odours have been thrown into the atmosphere to banish or destroy it—systems of laws have been enacted to bar its entrance into cities and countries—cordons of soldiery have been established to arrest it in its march—and processions, and fasts, and penances have been performed, to procure an exemption or a respite from its calamities. But, of its origin, nature, and mode of operation on the human system, no satisfactory views have been presented.

Of all the notions on this subject, none has been, nor ever can be more false and inconclusive to us, than that contained in the dissertation before us. This hypothesis alleges that contagion makes its way into the blood from without—mingles intimately with that fluid—and by a certain undefined fermentative process or some kind of intestine motion assimilates the general mass to its own nature, and through that medium communicates disease to the solids.

On the practicability of any acrimonious substance gain-

ing admission into the blood from without, we have already spoken, we hope satisfactorily. To the evidence adduced on that occasion we might safely refer for a complete refutation of the present hypothesis. But further irrefragable arguments may be abundantly subjoined. If the matter of contagion finds its way into the blood, and assimilates to its nature the whole or part of that fluid, some evidence of the fact surely exists, and that evidence must be sought for in the blood itself. But our author will permit us to assure him, that it will be sought for in vain. With all his discernment we defy him to distinguish the blood of a patient affected with a contagious fever, from that of an individual labouring under a fever of any other description. In none of its sensible qualities does the blood of the one differ in the slightest degree from the blood of the other. The fluid, both in colour, taste, smell, consistence and lubricity, is precisely alike, provided the fevers be of equal intensity, and the examination be made in each at the same stage.

Yet, says our author, the blood of a patient labouring under a contagious affection must be necessarily itself the seat of contagion, because it may be made to communicate the same complaint to a person in health. The truth of this position we deliberately deny, and, for evidence in our behalf, appeal to experiment, the only source from which it can be rightfully and rationally derived.

Of all diseases, small pox is the most contagious. But, on the authority of reiterated experiment, we assert, that the blood of a person labouring under that complaint, will not, by inoculation, communicate the same to a person in health.

Of kine pox, measles, and lues venerea, the same thing is true. Neither of these complaints can be communicated by the blood, as we are assured from the very best evidence. Notwithstanding what has been said by Home, we believe that measles cannot be communicated by inoculation at all. Totally without effect, have we often tried the experiment with the blood, the tears, the mucus secreted

by the Schneiderian membrane, and the eruptive matter mixed in water.

These experiments we entreat our author to repeat. In conducting them, he will be careful to draw blood from his patients in such a way, as to prevent it from becoming adulterated by the matter of contagion, in its passage through the orifice. He will be equally careful to mark and faithfully report the phenomena that succeed each act of inoculation. With regard to measles, he will bear in mind, that it is an epidemic disease, usually prevailing and spreading by atmospheric influence. It will not, therefore, be sufficient for his purpose that the persons he may inoculate with morbillous blood, be afterwards *merely visited by the complaint*, as such an event may be the offspring of the *common cause*. To attain certainty, the attack must occur at the proper period after inoculation, and be preceded by such a train of phenomena as to render it undeniable that it proceeded from the operation. In the estimation of intelligent physicians, an attack taking place without these circumstances, can have no weight. If, under this cautious and judicious procedure, either, or all of the foregoing diseases be regularly produced by *sanguineous inoculation*, we will surrender, without the least reluctance, the ground of controversy, and frankly acknowledge the error we have committed. But, should the case be otherwise, we have a right to demand similar concessions.

To the subject of true plague, the preceding remarks, in relation to measles, are perfectly applicable. We cannot credit that this complaint has ever been propagated either by sanguineous or any other mode of inoculation. We do not indeed regard it as a contagious disease. It is only a variety of the American pestilence, or yellow fever, and, like that complaint, prevails only *as an epidemic*—a pestilential condition of the atmosphere, and not contagion, being its source. To say that yellow fever is not contagious, is only to assert a truth, which has been long established by an array of evidence approaching as near to demonstration as the nature of the subject will admit.

In those instances where pestilence is reported to have been communicated by inoculation, we have no detailed account of the cases. We are neither told at what period after the operation the attacks took place, nor are we permitted to know what regular train of phenomena, or whether any, preceded such attacks. Did inflammation occur in the spot of inoculation? If so, was it *carbuncular*, or of any other *specific* character? Did the attack of disease that followed invade the system in such a way, as to render it obvious that the insertion of the reputed contagion was its source? Till these things shall have been made known to us, we lack information, and are at liberty to suspect, that the complaint was the offspring not of inoculation, but of the common epidemic cause.

Our author informs us that, in various instances, the fetus in utero has been known to sustain attacks, sometimes of small pox, and at others of lues venerea—and that, therefore, the contagion of these complaints must have been necessarily in the blood of the mother. The fact we admit, though not the inference here deduced from it. On the contrary, we regard it as speaking a language not only different, but directly opposite—as affording evidence of the highest probability, that the blood of the mother was *not* the repository of contagion. It is now known to every physiologist, that between the blood of the mother and that of the fetus, there is no *immediate channel of communication.* Each being has a system of vessels, perfectly distinct from that of the other, in which its own blood circulates. From the blood of the mother, therefore, contagion can no more pass *directly* to that of the fetus in utero, than to the blood of the infant after birth. To pause for the collection of evidence in proof of this would be a waste of time.

Congenital small pox is communicated in a different way. In every respect, *except circulation*, the fetus in utero is as much a *part of the mother*, as any viscus belonging to her body. Precisely as the disease makes its way throughout her own system, does it pass to the fetus—we mean by sympathy. Could we dwell on the subject, the existence

of this powerful tie between the mother and the fetus might be rendered demonstratively clear. On no other ground can the communication of parental likeness, or of hereditary diseases, be explained.

Shall we be asked, what is the source of the augmentation of the matter of contagion in the system, if it be not a fermentative process, or some intestine commotion in the blood? We answer, *secretion.* Every variolous pustule is a morbid gland, secreting from the uncontaminated blood the poison of small pox. The vaccine pustule is also a morbid gland, secreting the peculiar virus of that disease—the chancre is a venereal ulcerous gland, producing the specific virus of syphilis—and the capillaries of the skin generally, or of the Schneiderian membrane, or both, become diseased glands for the specific secretion of the poison of measles.

Can it appear to any one surprising or incredible that these poisonous articles should be secreted from unadulterated blood?

The blood of the rattle-snake, the viper, the mockason and the scorpion, is as free from contamination as that of man. Yet the deadly poisons with which these reptiles are armed, is secreted from their blood. The sap juice of the laurel and the mancinella is as pure and harmless as that of the oak or the chesnut. Yet from the former is derived, by the process of secretion, very active poisons. The same thing is true in relation to innumerable plants and animals that might be named.

Were it not for an apprehension that we have already unwarrantably trespassed on the patience of our readers, we would now proceed to an examination of the more local or insulated errors, into which our author has seemingly fallen. But, multiplied and glaring as they may be, and however emphatically they call for refutation, we must pass them over, and be content with what we have done for the present.

In animadverting on this Thesis, we have hitherto had to employ, which is always exceedingly painful to us, the language of disapprobation. But our censure is confined solely

to the doctrines it inculcates, about which, a difference of opinion may be fairly entertained and expressed. There is reserved for us, in closing our remarks, the pleasing duty of declaring that it has many redeeming merits. We have rarely read an early production of the kind which displays more recondite research, more dexterity of statement, more ingenuity of argument, or plausibility of style and manner. It is an able defence of the cause it espouses, and affords an earnest of future excellence, which no one will more delight to welcome and celebrate than ourselves.

ART. XII.—*Journal of the Academy of Natural Sciences, of Philadelphia.* January, 1821. pp. 32. DOBSON AND SON.
The same for February, March, October, November and December, 1822. *In all,* pp. 216.

OUR review of this work will probably be subjected to the perusal of two different classes of readers: the one fond of the study of Natural History, the other paying little attention to it, and conceiving it not particularly interesting. To most of the prior class this Journal is already known, and it is to that of general readers that it is principally necessary to address ourselves. They may perhaps find the subjects treated of more interesting than they were at first aware, and many of them may feel a patriotic interest in the success of a native work, considered by competent judges to possess merit of a high order, but which has hitherto languished for want of sufficient encouragement.

Natural History, in the extensive and accurate state which it has assumed in modern times, has been obliged, in all parts of the world, to depend principally for support, on the enthusiastic attachment of a few individuals. A judicious and entertaining selection, like that of Goldsmith,

could no doubt be made to amuse the public generally, and secure a very wide reception. But these are but the blossoms of a field, of which the harvest is gathered with toil and stored with care. The great task of studying and recording the organized forms and phenomena of nature, falls in all its weary weight, unmitigated and unparticipated, upon the shoulders of the naturalist. No dreams of poetic fancy, no classic recollections of former ages, no anticipations of future wealth, enliven the path of his labours. His fame is known but to few, and his cheek grows pale and his locks grey, amid the damps and shades of academic retirement.

Confined thus to the simple love of nature for their motives, how ardent must that love be, to persuade men to undergo such severe toils, and make such mighty sacrifices! How strong an affinity to the human heart must such studies possess! That such an affinity pervades every mind, there needs no proof. There breathes not the man whose soul is not delighted by the charms of vegetable and animated nature. The beauties of a fine landscape, or the gambols and instincts of animals, are objects enjoyed by all ranks, all sexes, and all ages:

> The prince, the clown, the warrior and the sage,
> And boastful youth and narrative old age.

This, indeed, in most minds, alternates with, or is subdued by the various feelings which arise in the business of promiscuous life. But in that of the naturalist, their effects are different. One strong, pervading sentiment fills his whole soul, and excludes all ordinary influences. He is content to forsake the pursuit of wealth, and even of fame, a much more kindred object, if he can only be allowed to continue, undisturbed, his daily and nightly study of the works of God. Hence the uniform benevolence and mildness of character which such employments generally produce. This strong desire urges him to traverse distant lands, to suffer the privations of barbarous countries, and to undergo with cheerfulness any fatigue, any tedium of dry and detailed research, or any disgusting operations, we may add,

of the dissecting room at home. No obstacle appals, no time seems long, and the toil of days or weeks is amply rewarded by the acquisition of a single fact.

The agency of these tempers together with the division of science, have increased its extent so greatly and embraced within it so many details, that observations which are interesting to the professed student, and valuable additions to the general sum of knowledge, are often not amusing to the ordinary reader; unless, indeed, as is frequently the case, he catches a portion of the flame. The public mind is only struck with generalities, or with facts embracing principles of some extent in their application; and the descriptions of particular and obscure species of natural productions, or the minute history of processes that are not conspicuous to the general eye, though undoubtedly of the greatest value to science, and implying the highest merit and the most solid qualifications, are generally overlooked, as requiring too much time and patience for the recompense they afford. Dry, systematical works on natural history, are perhaps of all books the farthest removed from that fluency and facility of being apprehended, which constitutes the most important quality of a good style.

We have often wished, when looking over the work now before us, that it had not been chastened down to such severe technical brevity. Were it otherwise, were it occasionally diffuse, like the treatises of Buffon, and in later times, those of the Dictionnaire des Sciences Naturelles, the work might expect a considerable patronage from the miscellaneous reader; and in works merely scientific, which are, as every one knows, so hard to be supported in this country, this is particularly an object by itself. Thus the numerous articles of a more generally interesting kind, which are contained in professor Silliman's Journal, must without doubt contribute materially to the support of that invaluable work. The effect too, on the minds of unscientific persons, is not without much importance. If it be desirable that love for the study of natural history be

diffused as widely as possible, why place its most repulsive form at what must be to many the threshold of the temple? Why announce a new animal exclusively in terms that require a previous acquaintance with the works of Lamarck or Cuvier; authors difficult to procure and to many inaccessible, (thanks no doubt to the wise tax levied on their importation,) and behind the very serious and inconvenient bar of a foreign language? Is not the example of the great works above alluded to sufficient to justify using a more intelligible and even popular style in treating these subjects?

To this, some will reply, that the power of teaching natural history in this pleasing form is a rare endowment of genius—that it requires the talents of extraordinary men to succeed in so difficult an undertaking. Of this we are doubtful. The subject, when in a form capable of being comprehended without difficulty, is in itself highly interesting, and needs only plain expression. But, whether it require a rare genius or not, we have evidence that individuals among the contributors to the Journal of the Academy possess this faculty, though possibly unknown to themselves. Examples of this talent may be found in several essays in this and the two last half volumes of the work. We may also refer to a late book of travels* of uncommon interest and of national importance, in the preface to which, the author informs us that the work is by no means an amusing one, that it contains a mere detail of some facts which he deemed of importance, and that it was published solely to prevent useful information from being lost, without any view to pleasing the public taste.

In expressing our wish that the contributors to the journal before us would conduce to the better support and patronage of their own work, and to an increased diffusion of scientific taste and knowledge in the country, by the additional insertion of more discursive and explanatory matter, we do not wish to be understood as willing to undervalue the terseness and brevity of their descriptions. These gen-

* Nuttall's Tour to the Arkansas territory.

tlemen write for the scientific world, and we can well admire the very large amount of new and valuable information thus transmitted to it, within the compass of a few pages. Respect, in no small degree, is unquestionably due to a work, which, in this ostentatious age, appears in the modest form assumed by the labours of the Academy, and which so well sustains its pretensions.

Anna Seward informs us that when the honest inhabitants of Litchfield heard strangers inquire the state of the botanical society of that place, who translated the genera of Linneus, and whose reputation was widely diffused through the kingdom, they stared with surprise, and acknowledged that they were not aware of its existence. Equally unknown to many of its fellow citizens, is a society whose membership is now acknowledged with pride by no small portion of the rising men of science in Europe. We shall now proceed to mention the origin of this association, which has continued, in its vigorous infancy, to add to the the mass of useful knowledge, and to do honour to our country.

On the 25th of January, 1812, several gentlemen, fond of the study of natural history, met at the house of one of their number, "to form a constitution, and become a society, for the purpose of occupying their leisure occasionally, in modes conducive to general satisfaction, and to the advancement and diffusion of science." Their names were John Speakman, Camillus M'Mahon Mann, M. D., Gerard Troost, M. D., Jacob Gilliams, John Shinn, jr., and Nicholas Parmentier, M. D. At the same time, Thomas Say, though absent, was considered a member. A committee was immediately appointed to take preparatory measures for commencing a library, by procuring periodical works. Within six months, the following gentlemen were elected resident or corresponding members: viz. Drs. Archibald Bruce, Samuel L. Mitchill, Augustus Muhlenburgh, Daniel Drake, and Augustus E. Œmler—also, Andrew Ellicott, S. Godon, J. Neef, Isaiah Lukens, and William Maclure. The association continued to meet, at first at each

others' houses, and afterwards at two successive rooms, hired for the purpose, till August, 1815—when they removed to their present hall. An act of incorporation was procured, bearing date, March 14th, 1814.

The hall of the Academy is a handsome though plain brick building, of a basement and two additional stories, situated at the head of a small court, running from Mulberry, above Front street. It contains, besides lobby and staircase, only one large room on each floor: that on the first floor is the ordinary place of meeting and contains, in addition, a library of great value. The second floor is appropriated to the museum. The institution owes much to the liberality of several of its members. Large donations have been made in various ways, and in particular by its president, Wm. Maclure, esq., who has furnished it with rare and costly books, as well as some apparatus, to a very large amount. It has served since as a place of reference and deposit for a great variety of new and rare productions. Whenever a curious point in natural history is to be settled, the identity of a doubtful species ascertained, or a new one made more completely known, the best method to effect these purposes is often thought to leave specimens at a public place, and one well known to naturalists, for the purpose of inspection and comparison. In this way a large addition has been made, and is weekly accumulating, besides the collections purchased at different times.

The ordinary meetings, which take place three, and occasionally four evenings in each month, are generally occupied with hearing read the written articles intended for the journal, frequent verbal communications of a scientific kind, with conversation, and occasionally lectures—and in receiving donations for the library and museum. The place thus forms a point of collection, comparison, and encouragement for every thing relative to the natural sciences.

In *zoology*, a great deal has been done, and much presented to the Academy, principally owing to the labours of Thomas Say, among the invertebral animals. This gentleman has described very many species, said by authors to

exist in America, but ill distinguished—and has enriched science by the discovery of a large number in addition, which were previously entirely unknown. He has published, in the two preceding half volumes of the Journal, a complete account of the crustacea of the United States. Much too, has also been contributed by C. A. Lesueur, particularly among fishes and mollusca. Contributions to this department have also been made in the first volume, by Dr. George Ord, Dr. Jacob Green of Princeton, Dr. Wm. E. Horner, Dr. Saml. L. Mitchill of New York, C. S. Rafinesque of Lexington, and Jacob Gilliams. We are indebted to two scientific friends for assistance in giving an account of many of the particulars we are about to enumerate, to whom we are not permitted to address our thanks by name.

In the present half volume, we have a *description of a new genus, and of several new species of fresh water fish, indigenous to the United States; by C. A. Lesueur.* The author informs us, that

"Messrs. Maclure, Ord, and Say, from their voyage to Florida, and Mr. Nuttall, in his last journey up the river Arkansa, brought back with them several species of fish, which it is his desire to communicate to this society. Several of them are undescribed, and one of them appears to constitute a new genus, allied to *Cyprinodon;* if we may admit for distinctive character the form of the body, that of the fins, their position, and particularly that of the anal one placed exactly between the ventral fins; the last of which characters appears to me of peculiar importance; it has likewise four or five branchial rays, and the remarkable teeth of Cyprinodon, whether or not they exist in the pharynx, as in that genus, he has not been able to ascertain."

The other species of fish which form the subject of this memoir, appertain to the genus Pœcilia, Schneider, and Lebia of Cuvier.

The new genus is distinguished by the essential and remarkable characteristic in this family of fish, of having the anal fin placed between the two ventral fins. This genus, under the name of Mollinesia, is dedicated to M. Mollien, French minister of finance, and one of the patrons of the celebrated naturalist, Peron. The species, from fresh water ponds, near New Orleans, is called *latipinna*, from the great

volume of the dorsal fin. Two new species from East Florida, belonging to the genera Lebia and Pœcilia, families composed of fishes commonly known by the name of *minnow* or *minney*, are described under the names of P. multilineata and L. ellipsoidea; as also one from the river Arkansa, about which some uncertainty remains.

Description of two new species of Exocetus.
By C. A. Lesueur.

The Flying-fish are distinguished among the abdominals by the uncommon magnitude of their pectoral fins, sufficient when extended to support the body for some seconds in the air.—Their flight is never very long, and they elevate themselves in order so escape the pursuit of voracious fish; they immediately fall, because their wings merely serve the purpose of parachutes; the birds also pursue them in the air, as the fish do in the water. They are found in all the temperate seas.

E. fasciatus and E. Nuttallii are described. The latter—in honour of Thomas Nuttall, the botanist, who obtained it in the gulph of Mexico, is distinguished by a remarkable fringe on each side of the mouth. They are both of small size.

Descriptions of the Thysanouræ of the United States.
By Thomas Say.

It would be, perhaps, no easy task to give the general reader an exact idea of the kind of animals that are immediately indicated to the zoologist, by the term Thysanouræ. We may, however, succeed with some persons, by stating that the small animal often seen running about on books in our libraries, especially such as are in damp situations, in form somewhat resembling a fish, and to which the name of *Silver-fish* is applied, belongs to this section of insects. The *Machilis variabilis*, newly described in this paper, may be seen by raising rocks and stones in humid places. The species Podura fasciata, P. bicolor, P. iricolor, and Smynthurus guttatus, are here first made known. They also frequent humid situations.

A notice concerning the Spider whose web is used in medicine.

By N. M. Hentz.

The narcotic quality and power in preventing intermittent fever, attributed to the web of a species of spider, are familiar to our readers. It is of importance to identify the kind. The author of this piece informs us that the animal employed in Philadelphia, is a new species, and totally distinct from the aranea domestica, which has been used in Europe; with which it is not even a congener. "It belongs to the genus *Tegeneria* of Walckenaer; the species of which form a horizontal web, with a cylindrical tube, in form of a funnel:" and which contains "the different species of spiders, which inhabit cellars and dark places." The spiders which make their web in the fields, on bushes, do not belong to this genus; "to ascertain which is the great point, as it appears that the web of all species belonging to it has the same virtues." "Its characters are: eight eyes, forming two parallel lines, the upper being curved and longer. Lip wider in the middle, cut straight at its extremity. Maxillæ inserted upright, not bent on the lip. Corselet nearly as large as the abdomen. The first pair of legs the longest, the fourth next, then the second, and the third the shortest." The *Tegeneria medicinalis* inhabits cellars, is of a black colour, inclining to blue; the abdomen marked with ten livid pale spots, and a line towards its anterior extremity.

An account of the Arachnides of the United States.

By Thomas Say.

By the term Arachnides we understand such animals as have the general appearance of the spider, including spiders, scorpions, ticks, &c. They exhibit a great variety of forms, and may be said with truth to be singular beings, both as respects their organization and their manners and habits. We are here made acquainted with a new genus of this class, an inhabitant of the ocean, under the name of *Anaphia*,

from α, without, and αφη, the touch, in consequence of its being altogether destitute of feelers or visible organs of touch. The whole animal exhibits, to the common eye, the appearance of a walking skeleton, rather than of a perfect being, endowed with the vital principle. Under the subsequent genus Buthus, we are informed of the existence, in Georgia and Florida, of a real scorpion with the poisoned sting in its tail. The animal which the negroes of the Southern States call scorpion, belongs to a very different type of beings. It was described by J. Gilliams in the first volume, under the name of Scincus erythrocephalus, and with us and in Europe would be called a lizard. The present paper includes the descriptions of thirty-three new species of animals, which we enumerate. Anaphia pallida, Buthus vittatus, Chelifer muricatus and oblongus, Phalangium vittatum, dorsatum, nigrum and grande, Gonyleptes ornatus, Trombidium scabrum and sericeum, Erythræus mamillatus, Gamasus antennæpes, spinipes, musculus, nidularius and juloides, Oribita concentrica and glabrata, Bdella oblonga, Ixodes annulatus, orbiculatus, crenatus, erraticus, variabilis, punctulatus, scapularis and fuscus, Hydrachna triangularis, Lymnocharis extendens, Leptas aranei and hispidus and Ocypete comata. The author states that he has a number of new species of Araneideæ, or true spiders, but that he withholds them, until he shall have extended his researches farther.

Description of several new species of Cuttle-fish.

By C. A. Lesueur.

The author describes the seven following new species, referred to the genera of the more modern writers, and to a new one, viz. Onykia, of his own; Leachia cyclura, Loligo Bartramii, L. Pealeii, from the Philadelphia museum, Loligo illecebrosa, used as a bait for cod, at Sandy Bay, Mass., L. Bartlingii, from the gulf stream, L. pavo and Onykia caribbæa, from the gulf of Mexico, and the gulf stream. He also gives a list of five unpublished species.

discovered by himself and Peron, in the eastern ocean, in order to note their existence, while he has sent descriptions of them to a Parisian journal.

Descriptions of the Myriapodæ of the United States. By Thomas Say.

The animals of which this class is composed, are commonly known by the names of "*thousand-legs*" and "*hundred-legs.*" They inhabit moist situations, under stones and other objects that have long rested loosely on the soil. The following new species are described; viz. Julus impressus, punctatus, annulatus, lactarius, marginatus and pusillus, Polydesmus serratus and granulatus, Polyxenus fasciculatus, Lithobius spinipes, Scolopendra marginata and viridis, Cryptops hyalina, sexspinosa and postica, and Geophilus rubens and attenuatus.

Observations on several genera and species of fish, belonging to the natural family of the Esoces. By C. A. Lesueur.

These fish are known in our markets by the names of pike, gar-fish, bill-fish, &c.; the two latter of which terms more particularly apply to the animals described in this essay.

"Mr. Cuvier observes, that the species of this genus are not yet well distinguished,* that they resemble each other so much, and present at the first view so little difference, that they all might apparently be embraced in the same description; that those who had observed them had been deceived by neglecting to obtain drawings, by which it would have been easy to observe their differences in a manner more sensible and more exact. It is from descriptions, and the comparisons of four different drawings from nature, made in different places, that I now consider myself authorized to distinguish three new species, which no doubt have been previously observed and considered as one and the same, existing throughout various seas,"

Viz. argalus, truncata, and caribbæa, all American animals. Two others are separated as distinct, from among

* Regne Animal, vol. II. p. 186.

those in the museum d' histoire naturelle, at Paris. Five new fish are given, under the genera Scomberesox and Hemiramphus, viz. S. equirostrum and scutellatum, and H. marginatus, baldo, and a variety of erythrorhinchus.

Description of Univalve Shells of the United States. By Thomas Say.

Most of the specimens described here were collected by the author, in the celebrated Missouri expedition. Sixteen new species are given of Helix, one of Polygyra, a genus previously established by himself, two of Pupa, four of Planorbis, eight of Lymnea, two of Physa, one of Cyclostoma, four of Paludina, five of Melania and one of Bulla.

Botany.

The writers on this branch of natural history who have contributed to the journal of the Academy, viz. Thomas Nuttall and Stephen Elliott, having both published elaborate works of their own, and being still engaged in continuing them, have had less to spare for the sheets before us. We have in the present numbers,

A Description of some species of Plants, recently introduced into the Gardens of Philadelphia, from the Arkansaw Territory. By Thomas Nuttall.

Descriptions of rare Plants recently introduced into the Gardens of Philadelphia. By Thomas Nuttall.

This gentleman has delayed publishing many of his newly discovered plants, until he has had opportunities of raising them from the seed and studying their habits, more at his leisure than in their native savannas; a modest and useful mode of proceeding, which does him honour, and affords a strong contrast to the improper and unbecoming haste with which some publications are made. He has here de-

scribed the beautiful Coreopsis tinctoria, already the pride of our gardens, the Helianthus petiolaris, Aster graveolens, Centaurea americana, Donia ciliata, Œnothera triloba, speciosa, linifolia and serrulata, Stevia callosa, Astragalus micranthus and Verbena bipinnatifida, nearly all of which are distinguished by their beauty, and some, as the Œnothera triloba, by the peculiarities of their growth, the elegant Nemophila phacelioides, and the Callirhoe digitata, a name now superceded by that of *Nuttallia*, conferred in honour of its enterprizing discoverer, by W. Dick, the cultivator of the small botanic garden attached to the University of Pennsylvania. Semper floreat.

On Geology and Mineralogy, in the first volume, there were contributions by William Maclure and Isaac Lea. In the present numbers, they have become much more numerous. Mineralogy has of late been the favourite study of a number of the members, and from the activity displayed by them in various ways an indifferent observer cannot fail to be struck with the rapid accessions to this science, which they are likely to effect. We shall proceed to enumerate the articles in the present numbers.

Observations on the Geological Structure of the Valley of the Mississippi. By Thomas Nuttall.

In an interesting paper of thirty-eight pages, this gentleman has had for his object to present the reader with a sketch of the observations which he has himself made on that important region. The well known talents for observation which he possesses, have enabled him to collect much valuable matter. His paper is divided into three parts, viz. "Observations on the probable Limits and Character of the Secondary Formation." 2d. "On the Limits and Character of the ancient Maritime Alluvium." 3d. "On the Transition Mountains of Arkansa."

In the first part our author observes that the great calcareous platform of the Mississippi valley presents the same characters, the same features as regard composition or

organic reliquiæ, the same zoophytic casts and impressions which we discover in what is called the compact mountain limestone of Derbyshire, in England, and that it is only the difference of their elevation above the horizon which in any manner distinguishes the same strata in our country from those of the other. He adds, "If we are then to search for a transatlantic region similar in its materials and in their horizontal stratification with the extensive plains of Ohio, of Michigan, Indiana, Illinois, Kentucky, West Tennessee, and a part of the territory of Missouri, it is to be in those extensive plains or steppes of the Tartarian deserts traversed by the Kuban, which have been described by Professors Pallas and E. Daniel Clarke."

Our author's observations extend over a considerable section of the Western States.

"In the summer of 1809, my attachment to the study of Botany, induced me to make a pedestrian tour round the greatest part of the southern shore of Lake Erie, to Detroit, from whence I proceeded in a canoe along the same coast of the Huron lake to the island of Michilimackinak, situated near its commencement. I then took a southwest direction along the coast of Michigan, to Green Bay; thence to the banks of the Mississippi, by ascending Fox River, near to its source, and embarking on the Ouisconsin, which disembogues itself two miles below the village called Prairie du Chien. I then descended to the town of St. Louis. This route, and the subsequent voyages which I made up the Missouri and Arkansa, afforded me an ample opportunity of instruction, as to the extent and character of this vast platform of secondary formation."

In the course of the first part, to which we invite the attention of those who are not acquainted with the mineralogy of the west, he describes the localities of the gypsum, salt, fluor, copper, lead, &c. which are found in many places.

In the second part are a few remarks upon the ancient maritime alluvium, and a flœtz formation, apparently connected with it, which continues from Rhode Island, to the coast of the Gulf of Mexico; principally with a view to ascertain its southwestern limits and to observe the in-

fluence which it has had in producing the present character of that part of the Mississippi valley.

Among the many interesting facts mentioned in this part, we notice one which strongly illustrates the common observation, that nature seems to have worked on a gigantic scale in the new world. We are told that the author found the ferruginous conglomerate resembling that of New Jersey to continue more than one thousand miles up the Red River.

In the third part he describes the hone stone of the Washita, and proposes to designate it by the name of Galactite. He doubts the existence of a vein of granitic rock of a red colour and containing very little mica, described by Schoolcraft as found at the lead mines of La Motte; which he asserts to be four or five miles wide, and which he traced for twenty or thirty miles. Mr. N. supposes it to be identical with the transition conglomerate which he saw at St. Louis.

Description of some new Crystalline forms of the Minerals of the United States. By Dr. G. Troost.

The progress which mineralogy has made in the last twenty years is undoubtedly to be ascribed to the importance which was attached to the crystallographical characters; for these can alone constitute mineralogy *a Science.* Although the study of crystallography was at first confined to France, still its influence was felt throughout Europe. The German and Scotch schools affected to despise it; yet, they became aware that it compelled them to be more guarded in the formation of new species, and that it was impossible for them to attach the same importance to their species as the crystallographers did, inasmuch as they admitted a passage from the one into the other, an idea altogether absurd in the eyes of the crystallographer. In neglecting these characters, the Wernerians found themselves, however, obliged to use the greatest precaution so as to avoid falling into inconsistencies. A careful examination

of other characters, particularly hardness and specific gravity, led them frequently to decide correctly of the nature of minerals. But the study of crystals, which was at first confined to France and England, has at last begun to force its way into Germany, and the Scotch school seems inclined to modify its system. In this country very little attention has as yet been paid to this subject, and the want of a good system of instruction, upon this plan, has been admitted by many of our mineralogists. Dr. Troost's paper is, we believe, the first one which has been published in this country which tends to add to the mass of facts collected abroad in support and extension of this method. The discovery of the zircons of Trenton and of the phosphate of lime of St. Anthony's nose, (N. Y.) under forms which appeared to him new, led him to examine them with more attention. He has found that these forms belong to the systems of crystallization peculiar to those species, and that far from presenting any discrepancy, they brought new facts in support of the science of crystallography. He has described these crystals in the manner of the learned professor to whom we owe the first clear idea of crystals. He has also given to them names connected with his system. The new forms which he describes will therefore rank in Haüy's method under the name of zircon *pyramidal* and *bisunitaire*, and phosphate of lime *unitaire* and *unitaire compressée*.

"It is found at St. Anthony's nose, near New York, in magnetic pyrites of a grey, sometimes bronze yellow colour. This ore is partly in a state of decomposition, having then the appearance of the brown oxide of iron. The crystals which occur in this part of the gangue have their edges generally blunted, which is not the case with those found in the unaltered pyrites. This would induce the belief that these crystals have been partly dissolved by the sulphuric acid formed by the decomposition of the pyrites."

We will take the liberty of here testifying our regret that Dr. Troost's modesty should have prevented him from giving his terminology in plain English. If Accum has

said, in his Crystallography, that the idioms of our language do not admit of adopting the whole nomenclature of Haüy; we consider this to have arisen from a fear lest the readers to whom he addresses himself would be deterred from undertaking a new science, by the multiplicity of words necessary to be learned in it. A similar principle has dictated the omission of all the more abstruse mathematical part of the system, in the same work; nor is this any reason why Dr. T. should not, as he does, avail himself of it. No one has, as yet, addressed to an English or American public, a work from which a writer could learn to construct or to understand a crystallographic formula, similar to those which the author employs. One writing with the authority with which the piece before us is penned, has, in our opinion, an indisputable right to translate into English, the foreign epithets, however newly coined, which are requisite to this subject; nor is there any danger that a reader previously acquainted with the science, would misunderstand them. Any one would, we think, prefer in the English language *bisunitary zircon*, to *zircon bisunitaire*, and *compressed unitary* to *unitaire compressée* or *unitaire compressed;* and we had hoped, that the honour of first employing the English language for this purpose was reserved for the United States.

Analysis of the Blue Iron Earth of New Jersey, made at the School of Mines at Paris, in the year 1819. By Lardner Vanuxem.

In the first volume of the second series of the American Philosophical Transactions, an analysis of this substance was published by Dr. T. Cooper, in which he supposed it to be a hydrate of the protoxide of iron. As the mineral presented all the characters generally attributed to compact phosphate of iron, these results excited considerable interest, and induced the author of this essay to repeat the analysis in Paris. The results which he obtained he deferred publishing until he had an opportunity of re-examining this

mineral, with the assistance of Judge Cooper, in the laboratory of Columbia College, in South Carolina, when his conclusions were the same as before. These analyses have shown, that the means which the judge used were insufficient to show the existence of phosphoric acid, which is one of its essential constituents. It was examined as an hydrated protophosphate of iron. We will not follow the details of the process employed; suffice it to say, it is the best which the modern improvements in chemical analysis could suggest. The author being desirous of obtaining the greatest degree of certainty, decomposed the phosphate of lime resulting from the precipitation of the phosphoric acid by the muriate of lime. He formed a phosphate of ammonia, which, being calcined at a red heat, gave him pure phosphoric acid. This he reduced by exposure to a red heat, with charcoal, in a coated glass tube, and obtained phosphorus.

Analyses of American Minerals. By HENRY SEYBERT, of Philadelphia.

A MINERAL was found some time since on the Brandywine, near Wilmington, (Del.) which all our mineralogists concurred in referring to Hypersthene. Its mineralogical characters are indeed as distinct as those of the Hypersthene from Labrador. The analysis of this mineral, given in the essay, induced the author to unite it with the amphibole of Haüy; in this determination, he was also directed by the fact of its fusing before the blowpipe into an opaque black glass.

We understand mineralogists are not all satisfied of the propriety of placing it thus, as some deem the analyses of amphibole and hypersthene, given in European works, too variable and uncertain to warrant such a conclusion, independently of other circumstances, and think the external characters of the stone sufficient to justify the previous arrangement. Of the accuracy and delicacy of the process, however, no one within our knowledge has ever doubted.

Next follows an analysis of a ferruginous oxidulated copper ore, which indicates much knowledge and skill in the analyst. It will interest all those who like to see science applied to objects which may become of use in the arts.

3. Of a green phosphate of lime (asparagus stone.) This analysis was made on a new and interesting variety of the phosphate of lime, and Mr. Seybert is, we think, the first who has determined the nature of this mineral. Its chemical composition is the same as that of the apatite of Europe.

On two veins of Pyroxene or Augite in Granite.
By Lardner Vanuxem.

In a very short paper of three pages, there is here described one of the most interesting geological facts which have come within our notice. We allude to the discovery of veins of pyroxene (the *triunitaire* crystals—the same form which are found in the lavas of the south of Europe) in granite. This gentleman has been led to the following conclusion; one of high interest to geologists.

"From the general character of these veins, their total dissimilarity with all rocks of the class to which the granite belongs, from their being composed of Pyroxene and of the *triunitaire* form so common in almost all lavas, I think in the present state of our knowledge, (as to the origin of rocks,) that we are in some measure authorized in considering them to be of Volcanic, rather than of Neptunian origin."

Description and Analysis of the Table Spar from the vicinity of Wellsborough, Lake Champlain. By Lardner Vanuxem.

This is the analysis of a mineral which had hitherto been very rare, having been found but at Oravitza and Dognaska, in the Bannat of Temeswar, and at Pargas in Finland; but which has been discovered in considerable quantities in this

country. Its real nature was not known until lately. It was generally supposed to be icthyophthalmite. The description and analysis here given, identify it with the European substance of the same name.

Geological and Mineralogical notice of a portion of the North-east part of the State of New York. By Augustus E. Jessup.

This notice makes known to us, one of the richest spots in minerals which our country has as yet afforded. The descriptions include twelve varieties of minerals, among which are the tabular spar, garnets, augites, sphene, &c. The author expresses his opinion that the bed of Lake Champlain rests upon secondary or flœtz limestone, and adduces several reasons in support of his belief that the secondary rock rests immediately upon the primitive, on the eastern as well as the western shore. After this, he proceeds to describe the situation and characters of Roger's rock, in which those interesting minerals are found. His paper conveys a good deal of information, and will no doubt direct the attention of all our mineralogists to the spot. This gentleman is already known to the public from his participation, as a man of science, in the Missouri expedition. His industry and skill in ground so new as America, cannot fail to be valuable to science.

In the other branches of natural science, nothing appears in the present half volume. They are, however, studied by different members connected with the Academy; but their labours have sought other channels. It may be doubted whether physical investigations can with propriety be included within the limits of the natural sciences. We think that the Academy, considering its title and objects upon an enlarged scale, should embrace them as being so included. There are only two communications upon physical subjects in the first volume; and as they both relate to apparatus for finding the specific gravity of minerals, they may be

classed by some, under the head of mineralogy. As chemical and mechanical communications have, within our knowledge, been made verbally, at the meetings of the Academy, and have been the subjects of conversation there, it is fairly presumable that they are considered by the members as legitimately within the bounds of their institution.

After giving this running list of the contents of the book under our hands, we are sure that much need not be said to induce that part of the medical public of the United States who read these pages, to form a just estimate of it. Our profession has been considered, in all ages, the natural patron of these sciences. It has furnished a very great proportion of those names which have become eminent by their successful prosecution. Our countrymen have not differed from the rest of the world in this judgment. Physicians are every where looked to, as their proper supporters. The journal of the Academy is a work which certainly deserves, if any book deserves, to be called national. It is the only journal in the United States, which is confined *exclusively* to original publication in these branches. Communications have been made to it from distant places; its fulness of matter is perhaps unprecedented, and the supply has been solely and unfortunately limited by the means of making it public. Its peculiar advantage, when it can be supported, is the celerity of its production, as by being produced in small pamphlets at a time, a discovery or improvement can be announced at a very early period after its occurrence. Its value to science has been too often acknowledged to be called in question. Can then the medical profession, can the public neglect such a work?

We have just seen, since the above was written, the first number of the new half volume designed for 1822. Its character is not inferior to that of the preceding. It contains, first, an account of a new mineral, discovered by professors Lardner Vanuxem and William H. Keating, at the deserted site of Franklin Furnace, near Sparta, N. J., and named in honour of president Jefferson. The *Jeffersonite* is a species of what professor Mohs has made a genus, by the name

of Augite Spar, and nearly resembles pyroxene. The mineral, on which great study has been expended, was found in a locality which offered a number of new and interesting varieties. A paper follows by professor Hare, having for its object, to account for the striking diversity of character between the northeastern and northwestern gales, which are common in the Atlantic States of North America. The author gives a beautiful chemico-mechanical explanation of the causes and peculiarities of these winds, and with his usual eloquence. We should be glad to enter more particularly into this and some other papers, but our task would not be prepared in time for the press.

Dr. Gerard Troost, after enumerating several singular but known forms among the beautiful quartz crystals of Lake George, proceeds, in a short paper, to describe and arrange a new one, by the name of Quartz annulaire, according to the system of Haüy.

Next follows a concise account, with a well executed lithographic plate, by C. A. Lesueur, of five new species of fish, of the genus Cichla; namely, C. *ænea*, *fasciata*, and *minima*, from Lake Erie, ohioensis, from the river Ohio, and C. floridana, from East Florida.

There is, next, the commencement of a paper by the indefatigable Thomas Say, upon the new marine shells of the United States. It includes two new species, Patella amœna and Fissurella alternata; when it is interrupted by the conclusion of the sheet.

We are pleased to observe on the cover of this number, that a London bookseller is formally associated in the sale of it. This will be serviceable, both by making more known to the British general public those merits which are already acknowledged by her men of science, and by increasing, in a foreign country, that patronage which is stinted in our own.

B. H. C.

ART. XII. *Medical Dissertation on Hemoptysis, or Spitting of Blood, which obtained the Boylston Premium for the year* 1818. By JOHN WARE, M. D., Fellow of the Massachusetts Medical Society.—Published by Cummins & Hilliard, Boston.

THE late Nicholas Boylston, of Boston, is the only individual in America, who has extended to medical science any liberality of patronage. He was not, we believe, of the profession; but duly estimating its importance, he bequeathed some years ago a sum of money, the interest of which to be appropriated annually as a premium, for the best dissertation on any subject connected with medicine. To the care of the University of Harvard, this fund is committed, and we understand, that the able professors of its medical school constitute a board, by whom the examinations are made and the prizes awarded. An arrangement such as this, might be expected to secure in all cases a just and enlightened decision, and the several dissertations thus distinguished which have already appeared, afford in their great merits, sufficient proof of it.

The one now before us, though, from the very nature of the subject, it may not claim much originality, is exceedingly well put together—it exhibits correctly the pathology of hemoptysis, and points out, in our opinion, the only course of successful practice. We know not, indeed, a medical tract much more pleasing or instructive.

Believing that the disease is not generally well understood, we purpose in the ensuing article, to enter into a pretty full discussion of its theory as well as management—in each of which views, it is highly interesting.

By hemoptysis is meant literally a *spitting* of blood. But as the term is commonly employed, it denotes a hemorrhage from the lungs, trachea or fauces. It is proper to know, that such bleedings are incident to these structures, and to be apprised of the signs by which they are to be discriminated. Much uneasiness may thereby be spared to our

patients, and the treatment of course, in the two cases, will be very different. Generally, when the hemorrhage proceeds from the trachea or fauces, there is merely hawking, without any pain, cough, oppression or febrile excitement—and, on examination, we shall, in some instances, be able to perceive the source itself, whence the blood comes. Now and then, it will be found, that the throat is inflamed, though more frequently, no such appearance is to be met with. The blood too, which thus escapes, is of a less florid or arterial complexion than when it issues from the lungs.

These hemorrhages, especially from the trachea, are sometimes of very serious import, and demand all our care. It has come within our personal observation, and similar occurrences have been noticed by other practitioners, that bleedings which could be distinctly traced to this part, proved to be the precursors of pulmonary consumption. In such cases it is more than probable, that the lungs are deeply affected, the diseased action extending to the windpipe, or, as may be, the irritation of the larynx, by degrees descends downwards, till the whole pulmonary system becomes involved. It has happened to us to witness such hemorrhages in the consumption which follows catarrh, and more than once, in the tubercular shape of the disease. But, on the whole, these bleedings are comparatively of little consequence, though, at all times, they should solicit our careful attention, in order to distinguish between what may be trivial, or otherwise.

With these remarks, we proceed to the consideration of the real hemorrhage of the lungs. There is here, a discharge of blood of a florid colour and frothy, raised by more or less coughing. It need not be confounded with hematemesis, the case which it most resembles. We are to recollect, that hemorrhages from the stomach are neither preceded nor attended by any pulmonary affection. There is no cough, dyspnœa, pain, nor oppression, and the blood is uniformly dark and grumous, generally mixed with the contents of the stomach, and is brought up by vomiting.

The causes of hemorrhages generally, may occasion hemoptysis. Yet there are some which more particularly conduce to this event. It is well ascertained, for example, that a predisposition to it, is laid in a certain conformation or structure of the body. Thus, a narrow thorax and prominent shoulders, a long neck with a delicate make, and sanguine temperament, seem particularly to invite such attacks —and this congenital, or any acquired predisposition, may be excited into action by a variety of circumstances. Not the least common or powerful of these, are long and loud speaking or singing, sudden bursts of laughter, violent paroxysms of anger, great exertions, and especially in the raising heavy weights, the vicissitudes of weather producing coughs, irregular habits of living, and the suppression of some customary discharge, as the hemorrhois or the menses.

But, though hemoptysis is excited by all the causes enumerated, it is still true, however extraordinary it may appear, that it occurs much more frequently at night, when of course there is the least corporeal or mental agitation. Of this we are certain, that of the cases which have come under our notice, a majority took place under the circumstances mentioned. Whether this is owing to an increased susceptibility acquired by the state of sleep, as has been alleged, we shall not take upon ourselves positively to determine. It is altogether a curious fact, and has never, perhaps, been very satisfactorily elucidated, or explained. We cannot, however, help suspecting that it is owing to the horizontal posture, and more particularly to the bending of the lower extremities, which unquestionably has great influence in determining blood to the lungs.

Of hemoptysis, there are several species or varieties, arising from different causes, and attended with various degrees of danger.

1st. It may proceed from the accidental rupture of a vessel, by a blow, a fall, or a wound, and here, if the vessel be small, and no previous disease exists in the lungs, it is a matter of little moment.

2d. It may occur from excessive inflammation of the lungs, as in the early stages of acute pneumonia, and here, also, it is not a very serious case.

3d. It may occur from a metastasis, or translation from some other part, as in suppression of the hemorrhois, the menses, or bleedings of the nose. Discharges of this vicarious nature are seldom prejudicial, where no predisposition to pectoral disease prevails, or unless they are very copious.

4th. It may occur, from mere plethora, or topical accumulation of blood in the vessels of the lungs, and which is relieved by periodical discharges. Cases of this description are not necessarily connected with the full and plethoric. They are as often met with in the thin and valetudinary, and though sometimes they may run into consumption, such is not invariably the result. Many, indeed, are the instances of persons, who, precisely in this state, have attained to a prodigious longevity: two or three remarkable examples of which have come under our own observation, and a very large number might be cited from various literary and medical records.

5th. It may occur from an abscess or ulcer in the lungs, most generally following ill cured pneumonic inflammation, the blood being mixed with phlegm or purulent matter. Even under such circumstances, though connected with great danger, the case will sometimes submit to a judicious course of treatment.

6th. It may occur from tubercles, associated as usually happens with the scrofulous diathesis, and, in this case, though rarely of any extent, it proves almost always fatal, by eventuating in pulmonary consumption, or as an effect of that disease already established.

As preliminary to the treatment of hemoptysis, we deem it proper to make some remarks on its pathology, and which, indeed, will apply to hemorrhage in general. The phenomena of active hemorrhage, bear a very close analogy to those of inflammation, and probably there is no essential difference in the two processes. In each, some change takes

place in the vessels different from that of health, and which induces an afflux of blood to the parts. Examined at this time, we shall find, in the case of hemorrhage, as well as inflammation, the evidence of irritation and congestion. The difference consists mainly, that, in the former condition, the vessels are relieved, at once, by effusion of blood, while, in the latter, by a more tedious operation, serum, coagulable lymph, or pus, is thrown out. Why the one or the other effect takes place, is owing to different states of the vessels, which need not here be explained.

Effusions of blood from blows or falls, or other accidents, are the result of an act of violence on a sound part, and if the injury be not great, by contusion or otherwise, is productive of no lasting mischief. But in real hemorrhage, the vessels of the part, being previously in a state of wrong action, unless the loss of blood be adequate to the entire subversion of that action, inflammation arises, which runs through its ordinary course, provided it be seated in certain structures. This is very much the case as regards hemoptysis. The lungs are singularly prone, from a variety of causes, to take on disease, and their bleedings are usually announced by a group of precursory symptoms, too often followed by inflammation, suppuration, and finally consumption.

That these speculative views may receive some further support, we shall, in the next place, inquire into the manner in which hemorrhage takes place. By the ancient pathologists, four modes were assigned: by the rupture of vessels,* by an erosion of their coats,† by transudation,‡ and by a species of oozing of the blood from the mouths of the exhalents.§

No doubt exists that hemorrhage occasionally happens from the first of these causes, and especially in cases from violence, as before noticed. But the present state of physiology forbids the idea of transudation in the living state, and not less so, of erosion of vessels from acrimony of the fluids. Whenever the latter is supposed to operate to the

* Rexis. † Diabrosis. ‡ Diapedesis. § Anastomosis.

production of this effect, it is, in all probability, owing to the removal of the surrounding substance, which gives support to the vessels.

It seems now to be the best opinion, that hemorrhage, as the more common occurrence, is owing to an effusion from the exhalents, which, by disease, are rendered more pervious, and permit through their patulous mouths this sort of leakage of blood. Of those who have most warmly espoused, and ably defended this hypothesis, the celebrated Bichat is not the least conspicuous,—and his arguments we shall now give in a citation from our author.

"He observes, 1. That in no instance, where he has opened the bodies of those who have died of hemorrhage, has he discovered any traces of erosion, although he employed the nicest care in washing and macerating the surfaces, and examining them with a microscope. 2. That in squeezing the mucous surface of the uterus in women who have died during menstruation, a number of small drops of blood may be pressed out, which manifestly correspond to the extremities of exhalent vessels: but no sign whatever of erosion can be discovered. 3. That hemorrhages sometimes take place from free surfaces, as the skin, in which the blood manifestly comes from the exhalent vessels: this renders it probable that the same is the case with the mucous membranes. 4. That if rupture always preceded hemorrhage, the internal surface of the womb would be a mere collection of cicatrices, as we must suppose one or more ruptures to occur at every monthly period of the catamenia. 5. That even if in active hemorrhages, where there is evidently a previous congestion of blood, we should admit the possibility of a rupture, how can we suppose it to take place in passive hemorrhage, where the powers of the vessels, almost destroyed by disease, permit the blood to pass freely from their orifices. 6. That it is difficult to reconcile many of the phenomena of hemorrhage—such as the extreme rapidity with which it is sometimes produced—its appearance in another part, when it has disappeared from that in which it previously existed, and its subjection to the influence of sympathy—with the supposition that it is produced by rupture. 7. The irregularity of the appearance of the blood in the course of menstruation, its copious flow in one instant and its complete cessation the next, and so alternately many times in the course of one period, are difficult to be accounted for on the principle of rupture, for we must suppose the wounds to be opened and closed again at every alternation of the discharge. 8. If we compare hemorrhages, allow-

edly proceeding from rupture, with others, they do not resemble them either in their phenomena or duration. Their cessation does not give rise to others: they are independent of all influence from sympathy—and the passions, which have a considerable effect on the common kinds, have none on these."

Did this reasoning of Bichat require to be sustained, it might readily be done by several other considerations. Deeming it, however, sufficiently conclusive, we shall not unnecessarily occupy time with details, which might run into prolixity. To what he has said, we will merely add, that the exhalents do pour out blood occasionally, is a matter of which our senses can take cognizance, as in petechiæ and vibices—in the passive hemorrhage of low fevers, and not less so in the instances of copious hemorrhage, happening in elevated positions, in consequence of the vessels becoming relaxed, from losing their ordinary support, owing to the rarefied state of the atmosphere.

Nor is the manner in which such effusions happen from the exhalents at all difficult, in our opinion, to conceive or explain. In a natural state, these vessels exercise the office of secreting, and throwing out a mucous, serous, or some more attenuated fluid. But becoming diseased, this capacity is lost, and blood passes through them unaltered. What renders this conjecture highly probable, is the circumstance of the blood, in some of these cases, being partially decomposed, a sufficiency of it only remaining to colour the mucus. We meet with this appearance very frequently in hemoptysis,—and dysentery affords an example still more strikingly illustrative. The discharges in this disease, which present every variation, from pure mucus to nearly pure blood, surely can only be accounted for by considering them as the effect of different degrees of alteration which the exhalents have undergone in the several parts of the intestines, or in the different stages of the case. Do we discover, we finally demand, any thing more here than in ordinary inflammation, where the vessels of the pellucid tissues are so changed, as to admit red blood? The fact, indeed, supplies us with new proof of what we have contended for, the intimate connec-

tion between the two processes of inflammation and hemorrhage.

In treating of hemoptysis, we are first to notice the more active species of the complaint, associated with an increase of vascular force, and febrile excitement. It may be collected from what we formerly said, that the degree of danger or curability will depend much on the condition of the lungs, in which the hemorrhage may take place. The case that now claims our attention, is ushered in by a sense of weight and oppression, and sometimes of slight pain or burning in the chest, particularly under the sternum—a dry, hard cough—some shortness and difficulty of breathing, increased by motion—tickling of the fauces—a full, irregular pulse, and a flushed countenance. In some instances, it is preceded by admonitions much less distinct—while in others, we have the unequivocal symptoms of the febrile condition, as chills, with the pallid constricted surface, coldness of the extremities, pains in the back and loins, disordered stomach, constipation and extreme lassitude, followed by much heat and excitement.

Cases of hemoptysis have indeed occurred, in which all the laws of intermittent fever were observed—the chill recurring with periodical regularity, and during the hot stage of the paroxysm, effusions of blood taking place—and in this way, continue for days, or even weeks.

There is a difference in the mode, as well as the quantity of blood discharged. It sometimes comes up in a very small portion, attended by rattling in the windpipe, as if the tube were stuffed up with phlegm, and is preceded commonly by a saltish taste in the mouth—by much irritation about the larynx, with an irresistible propensity to cough—which state of things may terminate with a discharge of a single mouthful of blood, or endure for a length of time—while in other cases it comes up without effort, and so copiously, as to appear like a stream from the mouth.

The leading indication in the case before us is sufficiently obvious, and consists in the reduction of vascular action. With this view, nothing would seem to promise so much as

venesection. But some have objected to the remedy, and among others, the late celebrated Heberden. It is deliberately put by him as a question to be solved by medical ingenuity, how the opening of a second vessel can check the flow from the one already ruptured? This is a sophism wholly unworthy of that great practical physician. Whatever might be the difficulty of explanation, he well knew, that the efficacy of bleeding had been demonstrated by very long and concurrent experience. But, really, it seems to us, that the case involves no dark enigma. By opening a vein in another part of the body, we invite the flow of blood to it, and thus, on the principle of revulsion, check the hemorrhage. The solution of the problem, however, is wholly immaterial. The fact of the utility of bleeding in the case, is admitted, and with this, we may at present be satisfied.

To be effectual in the more violent cases, it is indispensably necessary that the detraction of blood be large. The small and repeated bleedings advised by some practitioners are idle in themselves, and prove entirely unavailing. They harass and debilitate the patient, without at all contributing to his cure. It is our practice when called to a bad case of hemoptysis, where we think it expedient very promptly to effect relief, to take away at once so much blood as to make a decided impression, or in other words, completely to subdue the force of vascular action, if the flow be not previously checked. Nothing short of this will be effectual in the more copious and alarming hemorrhages of the lungs.

It is recommended by some of the authorities to restrict the use of the lancet, to cases only, that are marked by fulness and activity of the circulation, with vigour of constitution. That it is more urgently required under these circumstances, no one will deny,—though were the practice encumbered by such a limitation, the consequence would inevitably be mischievous. It so happens, indeed, that a large proportion of the cases of hemorrhage, and of active hemorrhage too, are attended not so much by redundancy of blood, as an unequal distribution of it, and

this in individuals, weak and valetudinary. Driven into the lungs in this undue quantity, which from their debilitated state, are unable at once to return it, and still more urged on by the vis a tergo, which this congestion increased, the blood must force itself out and continue to flow, while such a condition of things exists. To remove this topical accumulation, as well as to restore an equilibrium in the circulation, venesection is the appropriate remedy. We see it most strikingly illustrated in apoplexy and in many other cases. Nor in another view is the remedy less important. The lungs in hemoptysis are highly predisposed to take on inflammation, which, followed by suppuration, establishes consumption. Whether, therefore, with a view to immediate relief from the pressing evil, or as a measure of prevention against more serious mischief, depletion in the way and to the extent we have urged is imperiously demanded.

As soon as we have thus drawn blood, or even while the operation is going on, should the danger be great, we may direct common salt in a tea-spoonful or more, every five, ten, or fifteen minutes. Of the efficacy of this remedy in suppressing pulmonary hemorrhages, though originally a domestic or popular one, there is no reason now to doubt. Its effects are prompt and often very decisive. But how it operates, is not determined. It is more than probable, we think, that the action it excites on the fauces or larynx, is extended by continuous sympathy to the lungs, and constringes the patulous mouths of the exhalent vessels. It should always be given in substance, since in solution it is apt to excite nausea or vomiting, and is certainly less efficacious. Dissolved in the mouth and gradually swallowed, it excites a stronger impression on those parts with which the vessels of the lungs have the most intimate sympathy.

As further means in a critical case, we should resort to cold applications to the thorax, and particularly under the arm pits. No part is more susceptible than the axillæ, and such applications prove very serviceable. It has indeed been recommended in great emergencies, to wrap the whole body in a sheet wet with vinegar or cold water, or

even to resort to immersion.* This is pretty intrepid practice, which we have no doubt would prove efficient, though we have never seen it tried. The great objection to it which strikes us, is the danger of subsequent catarrh, or pneumonic inflammation, the lungs at all times being exceedingly intolerant of cold in whatever manner applied, and especially in a diseased state.

That of late, it has become much the custom to treat the case under review by the saccharum saturni, is well known. Long ago the practice prevailed in Europe, and is recommended by one or two of the earlier writers of this country. But in consequence of certain publications on the subject, by Sir George Baker, and other writers, it was entirely discarded. To the late Professor Barton, the credit is indisputably due, of dissipating these idle alarms, and for fully re-establishing confidence, so far as regards this country, in the safety and efficacy of lead. Perhaps, however, we have run into the opposite extreme, and now too promiscuously employ the remedy. It is too often prescribed, in every species of hemorrhage, and from whatever part of the body issuing. Yet it is certain, that the use of the lead should always be preceded by copious venesection, where activity of pulse exists, and this precept applies with peculiar force to hemoptysis. Of this we are assured, if venesection be not practised, the lead will always prove inefficient, and sometimes even detrimental. The just rule under such circumstances is, to deplete so long as there is much vigour in the circulation, and lead then brought in, will sometimes produce the happiest effects. The mode of giving it, is in doses of two or three grains, with a small portion of opium at short intervals. But such practice is only suited to hemorrhages originally slight, or have become so by the operation of previous remedies. To expect that it will restrain a large stream of blood from the lungs, is preposterous, and if it be employed with such a view, we shall always be dis-

* This was the practice of the late Dr. Thomas Bond, of this city, a most distinguished physician.

appointed. What would be the effect of a large dose of lead under such circumstances, is a matter of doubt. It ought to accomplish much more, and certainly it might be done with safety. On one occasion we gave twenty grains, without, however, any good effect. It is not at all unlikely, in our opinion, that, as in the case of mercury, the mode of action of this article is influenced by the quantity. The former, is a salivant, or purgative, according to the dose; and the latter may prove astringent, or otherwise, in the same way. We think so more particularly from having observed, in several instances, where very large quantities of saccharum saturni had been taken through mistake, that it operated altogether as a purge—none of its ordinary effects being displayed either immediately or remotely.*

To the vast utility of one of the preparations of zinc, namely, the vitriolic solution, in this hemorrhage, we have the evidence of Dr. Mosely, and of the late professor Barton, strongly and unreservedly expressed. Of our own knowledge, we can say nothing in its favour. Exactly on the same footing would we place the preparations of copper, so highly extolled.

To the remedies already mentioned, may be added some others, very fully accredited by no small proportion of practitioners. It is the narcotics to which we allude. The practice of using them on this occasion originated in Germany, and may be traced in the English medical writings. The principle which dictated it, is obviously to quiet irritation, and to subdue action by the supposed sedative influence of these medicines. No striking benefit, we presume, would result from either the henbane or hemlock, though strongly commended. But opium promises a great deal in these cases. What might be the effect of a very large dose of it, in the beginning of hemoptysis, is quite problematical in our estimation. That it has done good, when thus prescribed, we

* Colica pictonum, we are quite sure, never occurs from lead largely introduced into the stomach. It is caused by small portions of it, which are *retained*, and not *worked off*, as happens always in the other case.

are not without some direct proof. It would indeed be easy to cite many cases relieved in this way. Nor is the collateral evidence altogether to be overlooked. It is now admitted, that one of the most effectual remedies in certain cases of uterine hemorrhage, is to put the system completely under the impression of an opiate. But it will be asked, is not opium a stimulant, and can it be proper to give it, where there is an active and disturbed circulation? To this it may be replied, does not every surgeon, after an operation, to quiet the patient and prevent hemorrhage, resort to opium, though a very high degree of vascular action may prevail at the time? The fact is, that in speculating on the nature of this medicine, we should constantly bear in mind, that it is very peculiar. Most unquestionably, opium is a stimulant. To this point, the evidence is irrefragable. But this is so tempered by the other property, incident to it, of assuaging pain, and doing away irritation, that in many instances its effects are different, and may be safely and efficiently employed, where acting on general principles, it would be prohibited. Nevertheless, we are not prepared on our narrow experience to vindicate, to the full extent, the propriety of the practice which we have just stated. Wherever we have directed it in the early stage of hemoptysis, there has been great irritation of the pulmonary organs, attended with cough, &c. No one will dispute its propriety under such circumstances, and here we would, at present, let the practice rest.

What shall we say of digitalis? From its known influence over the circulation, it has been extolled in active hemorrhages, of every description, or wherever seated. But our own experience tells us, that as a substitute for the lancet, for which it has been proposed, it is totally ineffectual, and ought never to be trusted. Even where arterial action is reduced by direct depletion, it has always appeared to us very precarious in its effects, and decidedly inferior to many other remedies. Exhibited in the ordinary dose, much time elapses before the pulse feels its influence, and, if we increase largely the quantity, vomiting is endangered,

which, to say the least of it, is not a very prudent measure, in the more violent cases of hemoptysis. The fact is, that a great error has been committed in the application of this medicine to pulmonary hemorrhages. The case to which alone it is suited is, where there is a slight, habitual hemoptoe, attended by cough, pain, a quick and somewhat active pulse, and considerable mobility and weakness. To bleed, under such circumstances, is inadmissible on several accounts—and here it is, that a judicious practitioner, who knows how to temper his remedies to the exact character and condition of the case, will recur to digitalis as a substitute for venesection. Exhibited under such circumstances, in doses so as just to affect the pulse, and keep it within its natural standard, we shall sometimes derive from it the most signal advantage.

Emetics have been used in these cases. These were originally introduced by Dr. Bryan Robinson, and though the remedy received the attestations of that and some other writers, it has long since been abandoned on account of its supposed temerity. Certain it is, however, that active vomiting has, in some instances, very promptly suppressed hemorrhages from the lungs, and especially when spontaneously induced. By our own experience, we are supplied with several facts to this purport, and many more might undoubtedly be collected. Yet the practice must be deemed hazardous, and can only be warranted in the greatest emergency, and where the ordinary, or more prudent course has failed.* Doubting, as we do, in common with practitioners generally, the propriety of the unqualified use of emetics in hemoptysis, we most cordially concur in the almost undivided opinion, as to their great efficacy in small doses. The

* It is proper, however, for us to mention, that it appears from good authority, that the late Dr. Willis, so celebrated for his skill in the treatment of mania, and especially the case of the late king of England, was in the habit of recurring to emetics in all cases of hemoptysis, and that he declared, the lengthened experience of nearly half a century had taught him to confide in them above all other means, as well on account of their *safety*, as efficacy.

whole of this set of medicines may, perhaps, be beneficially resorted to with this view. Emetic tartar is much employed, particularly in highly febrile hemorrhages, to which alone we think it adapted.

With the confidence which arises out of an enlarged experience, we can recommend the ipecacuanha, as a most important remedy, under such circumstances. Many years have we employed it, and in a great variety of cases, and the more we have seen of its effects, the better are we satisfied of its valuable properties. It should be so given, as to create a slight distress of stomach, which ought to be kept up for some length of time. The ordinary prescription is half a grain or a grain of it, with a small portion of opium, repeated, at shorter or longer intervals, according to the necessity of the case.

The articles denominated *refrigerants*, are one of the means appropriated to the cure of this hemorrhage. The neutral salts generally are of this description. But the most valuable of them is the nitrate of potash, or common nitre. No one remedy is more popular than this, and, hence, it is often vaguely and injudiciously employed. Nitre has not, in the slightest degree, the power of directly restraining hemorrhage, or, in other words, of astringency. To give it, therefore, while the blood is flowing freely, is utterly useless. Having, however, a manifest effect in reducing vascular action, and febrile heat and excitement, it is, to meet these indications, an important auxiliary to the lancet. Nitre may be directed alone, or, what answers exceedingly well, with the antimonial preparations, in minute doses.*

* We have sometimes employed nitre in hemoptysis, in such quantities as to attain its operation on the kidneys, and with great success.

"In omnibus morbis pectoris ad urinam spectandum." This is the language of a distinguished writer, by which he meant to express the importance of attending to the appearance as well as to the promotion of the urinary discharge in the pectoral affections. I cannot doubt that diuretics are eminently useful in many of the complaints of the lungs, and hemoptysis is no exception to the very general rule. Nitre is the best of them—acting probably as much by the direct reduction of vascular excitement as by the promotion of the urinary secretion. It should be given largely,

Not altogether dissimilar in their properties and uses to the preceding article, are the mineral acids. They are given with precisely the same view, though we have some reason to suspect that they are more astringent, and may be, on this account, recurred to, in order at once to suppress a bleeding. Whether these acids are endowed with such a power, to any extent, we will not say. But we think with the evidence before us, we should not rely upon them to the exclusion of several of the medicines we have detailed, the efficacy of which is incontrovertibly and confessedly established. Of the acids, the best is the sulphuric in free dilution.

As yet, we have said nothing of purging. This, however, on every account, whether we regard the reduction of vascular action, or the removal of the irritation which a constipated state of the bowels induces, is a most important remedy. Early, therefore, in the case, as soon indeed as the flow of blood is checked, we should resort to purgatives, and the saline articles are preferred. Being less apt to excite nausea, or bring on vomiting, than any of the rest, Epsom salts are to be directed. The castor oil, if the stomach be in a state retentive of it, answers well. Magnesia is sometimes selected: but, on account of its huskiness, which, by tickling the fauces, is apt to excite coughing, should be avoided, or if permitted, some acid drink, as lemonade, must be taken immediately after it, by which the objection is nearly obviated.

We occasionally meet with hemoptysis connected with much depravation of the primæ viæ, as is indicated by the furred tongue, morbid secretions, constipation, &c. Cathartics of a different character are required under such circumstances. We have sometimes found rhubarb to succeed admirably, though, on the whole, the blue pill, exhibited conformably to the directions of Mr. Abernethy, is more

and very freely diluted. The fact is not sufficiently known, that an ounce of this medicine, dissolved in a quart of water, may be taken daily, with perhaps less inconvenience to the stomach, than one fourth of the quantity in the ordinary mode, and with a vast increase of effect.

effectual. Considering the dominion which the alimentary canal exerts over every part of the body, in health and disease, and especially its influence in pulmonary hemorrhage, we cannot be too careful to remove its morbid conditions, and for this purpose the practice just alluded to is incomparable.

Co-operating with this general course of treatment, there are some local applications of too much consequence to be neglected. We have before noticed the utility of cold cloths to the thorax, and particularly to the arm-pits, as means of speedily checking the flow of blood. But, in every instance, where we find the hemorrhage to continue, or even where a tendency to it exists, we should make use of topical depletion, or dry cupping, or blisters. Medical men are not entirely agreed as to the best place to make the latter application. It is said by some, that blisters are most effectual on the wrists or ankles, by others on the back, though most prefer them on the chest. As regards ourselves, we cannot entertain the least doubt, that the latter is the proper position. It is, indeed, a point sufficiently proved, that the efficacy of all such applications, is exactly as their proximity to the part affected. The only exception is, where we wish to interrupt morbid associations.

The course best calculated to relieve pulmonary hemorrhage has now been stated. That it may prove effectual, however, due attention must be paid to some other circumstances.

1st. The moment we are consulted in the case, we should enjoin a state of rest, in bed, with the shoulders elevated, and the lower extremities extended, for reasons before stated.*

2d. The chamber is to be kept cool, and well ventilated.

3d. Company should be excluded, and the patient not permitted to talk.

4th. Diet to consist of small portions of demulcent drinks,

* Vide p. 394.

acidulated and drank cold. It is right that the stomach be not loaded, as through it the lungs become oppressed.

5th. The bowels to be kept in a soluble state.

On the whole it will be perceived, that there is no striking peculiarity in the management of active hemorrhage. As it mainly depends on an increased force of the circulation, all our remedies point to its reduction, and it is fortunate, that, while they produce this effect, most of them are no less calculated in their immediate tendency to suppress the bleeding, as well as to obviate its return. Bearing steadily in view the close analogy of these cases with inflammatory fever, both in their nature and treatment, we are supplied with a guide, which will lead most certainly to a successful issue, with the least difficulty or embarrassment.

In place, however, of the active hemorrhage of the lungs we have described, we sometimes meet with cases connected with a very opposite condition of the system. These are denominated passive hemorrhages, or, with more propriety, hemorrhages of feeble action, and are commonly found among the weak and valetudinary, and especially of scrofulous tendencies. But on this subject our author is entirely silent, and we shall endeavour to supply his deficiency with some remarks of our own. Whether such be the state originally, or the excitement has been reduced by previous treatment, nearly the same course is to be pursued.

The indication here is clearly to support the system, and through it, to impart tone and strength to the vessels of the lungs. To attain this end, we call into requisition the different tonics, among which the Peruvian bark has always held a high reputation. Most practitioners confess its utility, and there are some who have extolled it even with enthusiasm. Commonly it is prescribed alone, though its powers are improved by a combination with myrrh or the chalybeate preparations. Of the great efficacy of these latter articles in the weaker hemorrhages, every one, of the least experience, is apprised. There is, however, a choice. The muriated tincture is perhaps the best, though the sulphate of iron answers very well. It is here too that the alum is

very generally prescribed, and though undoubtedly it does better than in active hemorrhage, still we are not sensible that we ever saw any great advantage from it—and, on the whole, repose very slender confidence in its powers in any shape of *pulmonary* bleedings. Cullen, we are aware, differs entirely in this estimate of the article. But we confidently repeat what we have said, and appeal to pretty ample experience in our justification. The vegetable astringents are also employed, and particularly kino and catechu. They are given alone, or with alum. Our remarks on the latter article, when by itself, apply equally to the combination. Greater advantage may be derived from the mineral acids. To such cases they are more appropriate than to the active form of hemoptysis. Either the nitric or sulphuric, according to circumstances, should be preferred. The former answers best merely to suppress the bleeding, and the latter to rectify the morbid condition with which it may be connected.

These are the principal medicines suited to the case before us. But they may be aided by the habitual use of moderate exercise, especially on horseback,* by a nourishing diet, as milk and eggs, and by such drinks as are corroborative, without heating or exciting the system. The malt liquors sometimes prove very serviceable.

By pursuing a course such as we have laid down, correctly shaping the remedies to the particular circumstances of each case, we shall frequently succeed in accomplishing a cure. Yet, unhappily, in some instances, and particularly when it is connected with constitutional imperfection, hemoptysis leaves behind it a liability to a relapse on the slightest provocation. To guard against these repetitions

* Exercise, on horseback, is of the greatest utility. We have known it frequently to effect cures. But the system should be carefully prepared previously to its being taken. On this point great errors are committed. We have often known patients sent on a journey with an active pulse and even febrile excitement. The recurrence of the hemorrhage, with aggravation, must be the inevitable result of such mal-practice.

of attack, a system of prophylactic instructions should be carefully suggested, and undeviatingly observed.

1st. To avoid every cause of increased impetus to the circulation, by the mildest food, such as milk, the gentlest exercise, and abstinence from excess of every kind.

2d. To obviate, by every possible precaution, taking cold. Nothing is so apt to reproduce it as catarrh. It is also more obstinate, when thus induced, and generally entails very serious or fatal consequences.

3d. To watch with care, where a strong predisposition exists, the state of the pulse and respiration. Either pain or oppression, or any increased activity of circulation, is a sufficient ground of apprehension, and must be removed without delay. To effect the purpose, small bleedings are demanded, a still lower diet, a state of rest for the time, some cooling medicine, and, in short, the whole antiphlogistic plan, in all its parts. As a preventive, much good has been experienced from perpetual blisters, and these, where there is considerable topical affection, are to be applied to the chest. But, under other circumstances, they will answer very well on the wrists or ankles, acting by revulsion. We should here bear in mind, that even in atonic hemoptysis, though there be general debility, *local congestion*, with sometimes *inflammation*, exists, and as the removal of these states is of primary importance, the appropriate remedies, though *depletory*, are not to be timidly withheld.

4th. Exactly on the same principle a salivation should be tried in obstinate cases. The impression thus induced in the mouth, transfers the diseased action from the lungs, or by the more general and revolutionary operation of mercury on the system, it may entirely supplant the disease, substituting its own peculiar action in place of it. The exception to this practice is to be found in tubercular consumption, with which the use of mercury is utterly incompatible under any circumstances.

5th. Emetics very frequently repeated, are entitled to much confidence in the latter cases. They operate by di-

verting blood from the lungs, and thereby establish a more equable circulation, relieve cough and oppression, correct the morbid derangement of the stomach, dislocate, as it has been expressed, the disease from it seat, leaving nature and our remedies to operate with infinitely more success.

Conduct, however, the treatment as we may, hemoptysis will sometimes present itself, of a nature so stubborn, implanted, as it were, in the very texture and constitution of the lungs, that it will resist all our endeavours. Consulting, in such cases, we should recommend, as the very last resource, removal to a warm climate, and by a voyage. This has very often protracted life, and even effected permanent cures, when every thing else had failed, and under circumstances the least promising.

Concerning the treatment of hemoptysis, we have only one word more to say. The limits which we prescribed to ourselves when we commenced this discussion, have indeed been greatly transcended. But the subject is most important, and we were reluctant to omit any matters which might lead to a better practical understanding of it.

We believe that among other errors, which are too commonly committed in the treatment of hemoptysis, is an attention too exclusive to the mere suppression of the bleeding. An hemorrhage of this sort, creates at the time the utmost alarm in the individual himself, his friends, and attendants. Great efforts are immediately made to check the flow of blood, and this being accomplished, the general anxiety which previously existed subsides. Thus lulled into false security, the patient reverts to his former habits, without any plan of permanent treatment,—till again awakened to a sense of danger by a repetition of an attack,—and in this way he proceeds, till the complaint is often irremediably fixed.

Now the fact is, that such hemorrhages are, for the most part, the endeavours of nature to relieve the lungs of oppressive accumulations of blood, and if not excessive, which rarely happens, are decidedly salutary, as much so probably as epistaxis in the congestions of the brain. They may, it is

true, leave some coagula or clots in the cellular and bronchial structure, which sometimes do harm by exciting cough, and perhaps inflammation, and this is perhaps the main objection to permitting the effusion to continue. Be this, however, as it may, no sound practitioner doubts that the hemorrhage of itself is comparatively of little moment, and that the real object of attention is the correction of the morbid state of the pulmonary organs giving rise to it, and which, if not timely arrested, results too frequently in the full establishment of pulmonary consumption!*

* *Note.*—It is some time since this article was written. In reperusing what we have said of the use of emetics, we are disposed to think that our advice concerning them is not sufficiently precise. When hemoptysis proceeds from rupture of a vessel, and is copious, emetics must, in our opinion, be hazardous, and altogether unwarrantable. But, caused by anastomosis,—an oozing through the exhalents,—and, as we believe generally happens, the hemorrhage is small, they may, on the contrary, be recurred to with safety, and, for the most part, with decisive advantage.

MEDICAL AND PHILOSOPHICAL INTELLIGENCE.

ANATOMY.

Preservation of Anatomical Specimens.—Dr. Macartney, of the Dublin University, has for some time employed a solution of alum and nitre, for the purpose of preserving anatomical preparations. He finds that it preserves the natural appearances of most parts of the body more completely than spirits or any other fluid heretofore used. The proportions of the alum and the nitre, and the strength of the solution, require to be varied according to circumstances; and, in order to impregnate thoroughly the anatomical preparations, the liquor must for some time be occasionally renewed. The solution possesses such antiseptic powers, that the most putrid and offensive animal substances are rendered perfectly free from fetor by it in a few days.—*London Medical and Physical Journal.*

PHYSIOLOGY.

Examen, &c.—Examination of the Blood, and its Action in the different Phenomena of Life. By J. L. Prevost, M. D. and J. A. Dumas.—The principal object of the microscopical researches and experiments of MM. Prevost and Dumas was to ascertain the existence and nature of the red globules of the blood. These authors agree with Hewson and Sir Everard Home, as to the form and structure of the globules, but do not admit, with the latter, that the red globules undergo a rapid change after they escape from the vessel, or that the colouring matter which envelopes the central spherical body separates from the globule in thirty seconds after the blood has issued from the vein.

According to our authors, the globules are of various forms and dimensions in different animals. In all the mammalia they are circular; in birds and cold blooded animals, elliptical.

Connected with this subject of the difference of form and dimensions of the globules in different tribes of animals, the authors detail the results of experiments on the subject of the *transfusion* of blood, not altogether in accordance with some other facts which are on record. When the operation is pro-

perly conducted, it is quite possible, by means of transfusion, to reanimate a body in which life appears to be extinguished from hemorrhage. But, according to our authors, the blood thus employed must be taken from an animal of the same species. If the blood which is injected is from an animal of a different species, but whose globules are similar in form, but differing in dimensions, the animal is but imperfectly restored, and life can seldom be preserved more than six days. In animals which have been submitted to this experiment, the pulse becomes more rapid; the respiration continues natural; but the heat falls with a remarkable rapidity, when not artificially supported; and from the moment of the operation, the dejections become mucous and bloody, and retain that character till death.

If blood of circular globules is injected into a fowl, the animal dies in the midst of most violent convulsions, which can only be compared to those resulting from the operation of the most intense poisons; and in some experiments death has happened before the first syringe has been completely emptied. The blood of a cow, as well as that of the sheep, has been transfused into cats and rabbits; and, whether the operation was performed immediately after the extraction of the blood, or whether the blood was left at rest, in a cool place, during twelve, or even twenty-four hours, the animal has in a great number of cases been restored for some days.

Spermatic Animalculi.—The rage for microscopy seems again to be revived. We are by no means averse to it; for we think that every channel of inquiry, however extravagant the results, may convey some useful information to the public. The existence of animalculi in the semen of divers animals, has been asserted by some, and denied by others. Hamme and Leewenhoek, Hatzœchen and Needam, have each, in their turn, called the attention of the public to this subject. The observations of Spallanzani, by far the most accurate and philosophical, are too well known to need being repeated. Dr. I. L. Prevost, of Geneva, and Mr. Dumas, a pharmaceutical pupil, have, again, taken up this question; and have lately published a dissertation upon it, containing many curious experiments, which go to prove, in the opinion of the authors, that—1st, the existence of spermatic animalculi is unquestionable; 2d, that they are distinct, in their characters, from the *vermes infusorii*, with which they have been confounded; 3d, that they are the produce of the testicles only, and are to be found in that organ when the animal has reached the age of puberty; and, lastly, that they appear to constitute the active principle of semen, in the same manner as the globules of the blood are the indispensable elements of that

fluid.—*Memoires de la Societé de Physique et d'Histoire Naturelle de Geneve*, vol. 1.

SURGERY.

Gastrotomy.—A young man accidentally swallowed a silver fork, eight inches in length, in consequence of which the most threatening symptoms ensued, and the greatest alarm was entertained. Mr. Renaud, an able surgeon at Romans, (department of the Drôme,) with a view to afford the only assistance in his power to the patient, undertook the operation of gastrotomy; which he performed in the presence of several physicians of the faculty of medicine of Paris and Montpellier, and succeeded, with much dexterity, in extracting the fork. The young man is considered out of danger, and was expected to recover in a short time. *Tablettes Universelles*, Oct. 1821, p. 61.

Remarkable Operation.—Professor Gräfe, of Berlin, has removed one half of the inferior maxilla from its articulation; in consequence of which operation it became necessary to apply a ligature to the left carotid artery. The patient was a girl twenty-three years of age, who had had a bony tumour situated on the jaw ever since she was six years old, and had then grown to an enormous size. The patient supported the operation with great fortitude. The wound was dressed on the fifth day, when the patient was not only able to utter some words, but to take food without great difficulty.

THEORY AND PRACTICE OF MEDICINE, AND MATERIA MEDICA.

Cretinism in a small District of the Kingdom of Wirtemberg.—In the valleys of Kocher and Roth, where there is abundance of stagnant water, there are upwards of one hundred and sixty cretins, in a comparatively small population. One third of these are quite deaf. About three hundred adults, either male or female, are afflicted with bronchocele. It is worthy of note, that the inflammatory and exanthematic diseases, to which the inhabitants of this district are frequently subject, are seldom, if ever, accompanied with fever, and that no intermittent fever has ever been known to prevail among them, Cretinism, however, seems, of late years, to be on the decrease in these valleys, owing, probably, to the fostering care of government, who have endeavoured to ameliorate the lot of these wretched inhabitants. —*Allgem. Mediz. Zeitung.*

The Influence of Artificial Eruptions, in certain Diseases incidental to the human Body.—Dr. Jenner has been long impressed with the idea, "that every pimple with a vesiculated head has an errand to perform for the benefit of the constitution." The influence of pustular eruptions, whether naturally or artificially excited, in many diseases incidental to the human body, can only be despised by superficial observers. To those practitioners who are intimately acquainted with the important functions performed by the dermoid system, the consent existing between it, and the more important organs in the economy, is a fact so striking, that we often find them grounding upon it, their judgment, as well as treatment of particular diseases. What wonder, then, that he, who has outstripped all his cotemporaries in the consideration of a subject of such high importance, should now come forward and propound, for the curious reflection of the medical public, a question, once keenly agitated, lately neglected, and now again likely to be discussed.

It is well known that the application of tartar emetic to the skin produces a pustular eruption not unlike that of mature small pox—nor can it be wholly forgotten that, at one period, this sort of external application had been recommended, on the false principle of counter-irritating, for the removal of obstinate internal complaints. So far back as the year 1794, Dr. Jenner had directed his attention to this subject; but his experience, at that time, was much too confined to enable him to bring forward facts of sufficient interest for public information; that experience has since been extended, and Dr. Jenner, deeming the question to be one of importance, has brought together several cases which have either passed under his own eye, or those of others on whose testimony he can rely, in order to give it additional interest, and with a view to direct to it an appropriate share of public attention.

The subject seems to have formed, occasionally, the topic of conversation between Dr. Jenner and Dr. Charles Parry—and to the latter, he, therefore, addresses his dissertation in the form of a letter.

The first portion of the volume contains the details of eighteen cases, the result of which, as corroboratory of the efficacy of artificial eruptions in curing various complaints, are most satisfactory.

Case 1, Mania. Cerate with tartarized antimony rubbed on the inside of arms night and morning, which produced papulæ of some magnitude discharging a serous fluid about the third day. "The transition from derangement to health was almost inconceivable."—Case 2, Idiocy, with a carbuncular tumour between the scapulæ. Tartar emetic ointment applied to the nape of the neck, and the patient regained a sane state of

mind soon after the eruptions were produced.—Case 3, Phthisis with anasarca and an enlargement about the left side of the thorax. The tartar emetic ointment rubbed on this part until pustules were produced. Complete cure in six weeks.—Case 4, Spasmodic asthma. Tartar emetic ointment on the nape of the neck. Greatly relieved.—Case 5, Disordered state of the brain; inflammation of the right eye. Tartar emetic ointment rubbed on the left arm. Cured in THREE DAYS.—Case 6, Chronic hepatitis with enlargement and induration. Tartar emetic ointment followed by pustules. Recovered with *astonishing rapidity*.—Case 7, Diaphragmitis—difficult deglutition. Spontaneous eruptions, with discharge, on the legs. Progressive recovery.—Case 8, Hemiplegia, chorea, convulsive fits. Tartar emetic ointment in the line of the cervical vertebræ. Cured.—Case 9, Mania. Tartar emetic ointment employed. Cured.—Case 10, Mania puerperalis. Tartar emetic ointment inside the arm followed by eruption after the lapse of a fortnight. Recovered.—Case 11, Hypochondriasis—mania. Tartar emetic used with much advantage; but the use of it relinquished by the parents before the recovery was complete.—Case 12, Hypochondriasis. Tartar emetic ointment on the pit of the stomach. Recovered.—Case 13, Pyrosis, vomiting of blood. Tartar emetic ointment inside of the arms. Recovered.—Case 14, Hysteria gliding into insanity. Tartar emetic ointment rubbed on the arm. Recovered.—Case 15, Hypochondriasis bordering on insanity. Tartar emetic ointment. Greatly relieved.—Case 16, Hemoptoe. Tartar emetic ointment on the chest. Relieved.—Case 17, Pulmonary affection. Tartar emetic ointment on the chest. Great relief.—Case 18, Intense dyspepsia with pain. Tartar emetic ointment followed by complete recovery,

Such is the summary of the evidence brought forward by Dr. Jenner in support of his favourite plan; and it must be acknowledged to be of a very decided character.

Dr. Jenner next proceeds to comment upon a practice of which he ably traces the history; and, after hazarding a few physiological hints upon it, concludes with recapitulating the principal points of inquiry.

This letter contains the abstract of communications from Messrs. Fry and Fosbrooke, and from the author's own relative, the reverend A. Jenner, speaking favourably of the effects of tartar emetic on the skin in various diseases.

We have long been in the habit of using this remedial agent in obstinate diseases of children on a very extended scale, as the registers of the Royal Metropolitan Infirmary for children will prove. We know of no other application that will so effectually subdue mesenteric disease, as the one under consideration. Our plan, however, is not to order unctions, which,

being tedious operations, and, necessarily, to be repeated frequently, are seldom attended to by the parents; but we direct a soft cerate containing soap or ammoniacum, the surface of which is entirely covered with finely pulverized tartarized antimony, to be applied to the abdomen or scrobiculum cordis, and have thus seldom failed to produce large crops of pustules filled with a thick, yellowish, laudable pus. We have adopted the same practice, and with success, for the relief of diseased action in the cranial brain of children; and in our private practice we have recommended the application of external tartarized antimony, with great benefit, in a case of gastrodynia that had resisted every other medicine,—in another of hip disease,—and in a third of hepatic phthisis in which the life of the patient had been despaired of by two other physicians; but where the appearance of the pustules insured a recovery. The last case we attended with Mr. Willis, of Kennington.—*London Medical and Physical Journal.*

Pulmonary Worms.—Mr. Rogers has made a discovery, which may ultimately prove of some importance. He says, "in making some microscopic experiments with small spherules of very high magnifying powers, I have observed that the matter, or pus, expectorated in a certain stage of pulmonary consumption, is actually filled with multitudes of minute worms; the forms of which, in their evolutions from the surrounding mucus, are so distinctly seen, as to obviate all doubt of their identity with living animalculi. I am induced to hope that an attentive consideration of the subject, by those professionally competent to its investigation, may tend to throw a light on the (at present) inscrutable cause of this malady. Is it unreasonable to regard these worms, the existence of which is indisputable, as forming the concomitant cause of consumption? And may not the irritation, consequent on the motions and gnawings of so many thousands of these vermin in so sensitive an organ, account for the restless excitability of the parts, the ulceration, and some other incidental symptoms? I have observed the worms to be killed, while under the microscope, by exposure to a particular kind of gas; which it might be imprudent, in this stage of the inquiry, to name."—*London Medical and Physical Journal.*

On the Efficacy of the Bark of the Pomegranate Tree in Cases of Tænia. By P. Breton, Esq.—Eight cases of tænia are related, in four of which a decoction of pomegranate was employed, and in the others the powder, with equally good effect. The decoction was prepared by boiling two ounces of the dried bark in a pint and a half of water, until it was reduced to twelve

ounces. Of this two ounces were given every half hour, until four or five doses were consumed; when vertigo, sickness, and uneasiness in the bowels commenced. The medicine was then suspended, and at some time within twelve hours the worm was expelled. The powder was administered in the dose of a scruple to a boy, and two scruples to an adult, every half hour, for five or six times, when the same symptoms and effects resulted, as when the decoction was exhibited.

Use of the Piper Cubeba in Leucorrhœa.—Dr. James Orr, of Edinburgh, has communicated to Dr. Duncan, jun. four cases of leucorrhœa cured by cubebs. In the first case, the patient had laboured under the disease for seven months. In the second, the complaint had lasted nearly eight years; in the third, it was of two years' standing; and in the fourth, the discharge had resisted the use of several remedies for six months. In the first three cases, the disease succeeded to abortion. The pepper was administered either in water or milk, or made into an electuary with treacle. The dose was from one drachm and a half and upwards, three times a day, but never exceeded three drachms. —*London Medical and Physical Journal.*

Internal Hemorrhage, without any apparent Rupture of Blood Vessels.—A very rare instance of sudden death from internal hemorrhage in the abdomen, without any visible rupture of a blood vessel is related in one of the late numbers of Hufeland's Journal for 1821. A woman, aged thirty-three years, who had always regularly menstruated, had her menses suppressed, in consequence of catching cold and getting her feet wet. She fell ill the same evening, and died at noon, on the following day. On examination, no traces of inflammation could be discovered in any part of the body; but, in the abdomen, upwards of three pints of fluid blood were found without any evident cause being traced for such an occurrence. It is needless to state that the accident was attributed to the sudden cessation of the menses.

Treatment of Erysipelas.—It has been stated, on the authority of an American physician, that, in cases of erysipelas, partilarly of that of the face, the application of mercurial ointment to the inflamed surface is speedily followed by beneficial results. Mr. Brodie, having had occasion to put this practice to the test of experience, found that, although its utility seemed unquestionable, a very serious inconvenience attended it,—namely, salivation. Being struck, at the same time, with the idea, that the benefit, obtained in such cases, was due, more to the adipose substance employed than to the metallic oxide combined with it,

he imagined that the external application of a simple ointment might, probably, answer the same purpose. We have his authority to state, that this idea has proved correct in several instances of erysipelas, in which the simple ointment has been applied. The patient, after its application, has experienced an almost immediate relief; and the disease appeared to go through its various stages in a more favourable manner than under ordinary circumstances.—*London Medical and Physical Journal.**

On the Use of Belladonna as a preservative against Scarlet Fever.—The observations made, twenty years ago, by Hahneman, on the utility of belladonna in preserving from scarlet fever, has been confirmed by late experiments. In Hufeland's Journal, for August, 1820, there is a statement of facts in favour of that medicine. Dr. Berendt, (not Brandt, as some cotemporaries have stated,) an Austrian physician, gave, morning and evening, a few drops of a solution of three grains of the extract of belladonna in one ounce of water, to one hundred and ninety-five children exposed to the contagion of scarlatina; fourteen of whom, only, became infected with that disease, which they had in a very mild degree.

During the last epidemical scarlet fever which raged in Copenhagen, and throughout Denmark, (1821,) this medicine proved extremely useful. Professor Herholdt, physician to the principal hospital in that metropolis, and one of its most eminent practitioners, has used a weaker solution of the extract, (two grains to one ounce of water,) and found it to answer the purpose of preserving several hundred children from the contagion. He observed, however, that the common and well known effects of this medicine showed themselves in the little patients, when the dose of the solution, at any one time, amounted to twenty drops, and he therefore limited it to fifteen. The same author also prescribed it in a very recent epidemy; arrested, by it, the spreading of the contagion in families where the scarlet fever had made its appearance; and prevented its infecting most of the other families of which he was the attending physician.

* We have not seen or heard of any instance in the practice of this country of salivation thus induced, though we give full credit to the statement of Mr. Brodie. He, however, has been anticipated in the suggestion of the efficacy of the mercurial ointment proceeding from the adipose matter which it contains, by Dr. Colhoun, who in the fourth number of this Journal, published some cases of erysipelas, to show that hogs' lard is productive of equal utility. The correctness of the opinion we denied at the time, and urged in confutation of it the fact, that the *watery* solution of corrosive sublimate, and calomel *dusted* on the parts, are also very effectual in erysipelas. To treat this complaint with oily remedies, as for example, with simple cerate, was indeed the practice of Wiseman, and the older surgeons generally, which on the authority of Desault and the later writers, has been abandoned as injurious. EDITOR.

Having remarked that the medicine ordered in the form of drops was more readily neglected in large families, Dr. Herholdt directed the extract of belladonna to be formed into lozenges, with a sufficient quantity of chocolate; and of these, each of which contained 1-32d part of a grain of the extract, he administered from one to three every morning and evening, according to the age of the patient. Out of nearly one hundred families, in which he had occasion to use the belladonna, one only was attacked by the scarlet fever, during the epidemy, in spite of the remedy. It is not certain whether, in this family, the individuals had not been infected previously to their taking the belladonna, or whether they had neglected taking it regularly. Dr. Herholdt thinks that the medicine does not act as a preservative, unless it be given long before the person is exposed to the contagion.—*London Medical and Physical Journal.*

On the Strong Evaporating Property of Camphor, employed as a Remedy in several Diseases. By Dr. Bodtcher.*—It is a well known fact, that camphor possesses a high power of evaporation; and that, when exposed to the open air, it soon diminishes in volume, and ultimately disappears altogether. A curious circumstance, connected with this fact, first directed Dr. Bodtcher's attention to the possibility of its being applied to the purposes of medicine. Happening to enter some years ago the pharmaceutical dispensary of Fridrik's hospital in Copenhagen, while labouring under a very severe headache, some bottles containing camphorated mixture were brought in from the laboratory, and set on the ground not far from the doctor, who, after standing by them for a few minutes, found that his headache had entirely left him. This induced him to try the effect of the vapour of camphor, in other complaints, and particularly in those where the camphor itself, or any of its preparations, could not be well applied, as for instance, in complaints affecting the cavities of the nose, the throat, and chest. The success, said to have followed this new mode of employing camphor as a medicine, is most encouraging. In the worst cases of complete stoppage at the nose, (snuffles,) a piece of camphor need only be kept for a few minutes before that organ, and the patient will, in a short time, be relieved. In inflammation of the eyes it has also proved useful, when held at intervals before them. In cynanche, kept either before the open mouth or the nose, it has produced the best effects. As gargles with camphor, however useful in this complaint, are not always admissible, the knowledge of the fact mentioned by Dr. Bodtcher may be of great service to practitioners. In the latter

* From the Danish Journal, intitled, "Bibliothek for Læger."

disease, the vapour of warm water might be directed to be inhaled with some camphor previously mixed in it. The vapour of camphor has been found very beneficial in common and spasmodic coughs, and even in croup it would be of use, if resorted to in time. The same observation may be made with regard to all kinds of inflammations of the chest.

When gout and rheumatism attack the internal organs, this important remedial agent ought not to be neglected. Experience teaches us that, when camphor is applied to the external parts of the body affected by either of those diseases, the latter will often quit, suddenly, their original seats, and settle, not without great danger, on the internal organs. When this has been the case, the inhaling of the vapour of camphor, Dr. Bodtcher thinks, will be found of great service. In all such cases, it would, perhaps, be better to keep a small piece of camphor in the mouth, in order to secure the benefit of its evaporation.—*London Medical and Physical Journal.*

Use of Sub-nitrate of Bismuth in Intermittent Fevers.—Dr. Henkersew, a physician at Hildesheim, has been in the habit of prescribing this remedy in agues, for several years. He considers it to be a powerful febrifuge and anti-spasmodic. He exhibits this salt in the dose of four grains, with a few grains of sugar, every two hours.—*London Medical and Physical Journal.**

More Information respecting Iodine.—M. Heusmans read before the society of medicine of Louvain, at its sitting of the 16th of January, 1821, a paper upon the preparation of the tincture of iodine, and the re-establishment of that tincture deteriorated by time; as also on the non-existence of iodine in burnt sponge and in the ashes of the turf of Holland and the Netherlands. MM. Fyfe and Straub had announced the presence of this comburant in the ashes of Swiss turf.

As to the means of re-establishing the tincture of iodine, when by the decomposition of the alcohol it has passed into the acid state, it consists in infusing the tincture with an excess of super-oxide of manganese; the hydrogen is expelled, and the iodine regenerated; but the alcohol does not recover its primitive force, and is thus prevented from holding the iodine in solution. This tincture, which M. Heusmans exhibited to the society, was of a deep brownish red, it stained the hands intensely, and contained forty-eight grains of iodine per ounce of alcohol, at 35° B.—*Annales Generales des Sciences Physiques.*

* In the use of this article, the European physician has been anticipated, by Dr. Carmichael of Virginia. Vid. Chapman's Therapeutics, p. 484, vol. 2.

CHEMISTRY, &c.

Memoire sur les Teintures, &c.—Memoir on the compound Alcoholic Tinctures, Wines, &c. By M. GRAMMAIRE, Apothecary. —The point to which the observations detailed by M. Grammaire are principally directed, is, that in the pharmaceutical preparations of compound tinctures, the *mode* of preparation as well as the quantity and degree of concentration of the alcohol, influences in no small degree the nature of the product. He shows, by a series of experiments, that in the usual method of macerating the different substances together in the alcohol, we obtain no fixed proportions of active principles; and that the tincture thus formed, contains a third, fourth, and sometimes even one half less of resinous or extractive principle, than when each of the ingredients is separately macerated in the alcohol, and the several tinctures, thus prepared, are afterwards mixed.

The following, out of a great many trials, according to the new process and that ordered in the pharmacopœia, will illustrate and confirm the author's observations and deductions :—

TINCTURA CATHARTICA.

PREPARE ACCORDING TO THE CODEX.	PREPARE ACCORDING TO THE NEW PROCESS.
Take of Jalap Root, 2 drams and 2 scruples	Take of Jalap Root, 2 drams and 2 scruples
Turbith root, 1 scruple	Alcohol, 2 ounces
Scammony, 2 scruples	Turbith root, 1 scruple
Alcohol, 4 ounces	Alcohol, half an ounce
	Scammony, 2 scruples
	Alcohol, $1\frac{1}{2}$ ounce
Macerate all the substances for fifteen days in the alcohol.	Macerate, separately, for the requisite time, and then mix the several tinctures.

The quantity of extractive principle obtained by operating according to the first process, is 58 grains, whereas, in the new process, it amounts to 102. So that by treating the tinctures separately, we obtain a much stronger product; and by ascertaining the quantity of active principle taken up by the alcohol from each substance, we know precisely the relative proportions of the ingredients in the tincture; and consequently arrive at greater certainty and precision in the administration of such medicines.—*London Medical Intelligencer.*

Chemical Examination of Cubebs.—By M. VAUQUELIN.—Cubebs are the fruit of the piper cubeba (triandria trigynia), a perennial plant, which grows in the Philippine Islands, in Java, Guinea, and the Isle of France.

These grains do not all possess the same colour; they are rounded, and attached to a stalk. If they are examined after

being macerated in water, four coverings are discoverable; the first is fleshy, and softens in water; the second is of a grey colour, and is nearly transparent; the third is thin, like the peel of an onion, of a yellowish brown colour; the fourth is a very thin, white pellicle, immediately covering the grains, some of which are round, and entirely fill their covering; others are flat on one side, and rounded on the other; others, again, are wrinkled, and covered with fatty matter; and, lastly, there are some which are of a white colour.

A careful analysis shows that the cubebs contain the following substances:—

1. A volatile oil, which is nearly solid.
2. Resin, resembling that of balsam copaiva.
3. A quantity of another and coloured resin.
4. A coloured gummy matter.
5. An extractive principle, similar to that which is found in leguminous plants.
6. Some saline substances.—*Annals of Philosophy.*

On the heat of solutions crystallized by exposure to air.—When a hot saturated solution of Glauber's salt is suffered to cool in a closed flask or phial, it remains fluid until the stopper is removed, when it immediately crystallizes, and its temperature becomes elevated. Dr. Thomson has lately experimented on this phenomenon; not with a view of ascertaining the cause of the crystallization, but on the source of the heat evolved. Sulphate of soda and carbonate of soda were the salts used; 51 parts of crystallized sulphate of soda to 49 parts of water, make a good solution for the experiment. If carbonate of soda be used, one part should be dissolved in 4.22 parts of water, and the solution suffered to cool below 50° F. When the sulphate of soda is used, the crystallization of the mass is certain, at a temperature under 50°, on removing the stopper, and the rise of temperature equals 24° F. If the carbonate be used, immediately the cork is drawn, an abundant precipitation of small crystals in stars takes place, and the temperature rises 14° F. From estimation made of the rise of temperature; the whole quantity of matter heated; the quantity of salt solidified; of water of crystallization combined with it, &c. &c., Dr. Thomson concludes, that the latent heat of the water of crystallization was, in both cases, the sole source of the increase of temperature observed.—*Annal. Phil.*

Indication of the Temperature of a Room by two Thermometers at different altitudes.—Mr. John Murray has been induced, from some accidental observations, to make a series of experiments on the difference of temperature indicated by two ther-

mometers at different altitudes, though, otherwise, under similar circumstances; and was surprised to find that there existed an invariable difference between the temperature indicated by each, which he is inclined to consider to be an accurate guide for judging of the state of the weather. He placed one of the thermometers, with its ball, on the floor, and the other he suspended 6½ feet above the former. When the difference between the two exceeded 2°, to 2° 5′, F. the weather was variable and wet.—(*Phil. Mag.*)

MIDWIFERY.

Case of Morbid Occlusion of the Uterine Orifice during Parturition, and an Account of a sure and easy Mode of remedying it. By P. Moscati.*—Occlusion of the mouth of the uterus, total or partial, may be the result of congenital malformation, or of rupture or other injury from the employment of instruments during labour, and consequent cicatrization. Such obstruction may also arise from a change of position of the uterus in pregnancy, and the impracticability of restoring to the cervix its natural direction. Baudeloque, in such a case, unable to replace the uterus, tried to form in it an artifical opening; but the woman died from inflammation. Lauverjeat, in another instance, was more fortunate: he incised the body of the uterus on the prominence announcing the situation of the head of the fœtus, and thus procured its expulsion. The woman got well; and in two months after the uterus resumed its natural situation. But the partial occlusion of the orifice from cicatrices, consequent on laborious labour, is the more frequent occurrence. Of this nature is the following case, detailed by Moscati. A woman, aged twenty-five, had been lacerated by imprudent application of the forceps. The uterine orifice was so much contracted by the resulting cicatrix, as only to admit a fine probe. Pregnancy again ensued; and on the occurrence of labour, forty-eight hours severe pain effected no sensible dilatation. Profiting by the recollection of a former unfortunate case,† Moscati was deterred from the usual mode of operating, and thought of another simple mean; which consisted in introducing a slightly curved *bistouri caché* into the uterine orifice, and making slight incisions round the whole circumference of the cervix, and thus enabling the orifice to yield to the necessary extent. The incisions, practised during the labour pains, caused no additional suffer-

* Translated from a memoir presented to the Italian Society of Sciences.

† In this case, incision of the uterus, necessitated by occlusion of its orifice, terminated fatally. On dissection, it was found that the uterus had been ruptured to a great extent, in the direction of the angles of the incision, by the pressure of the head of the fœtus.

ing. The uterine orifice, after the patient had reposed awhile, dilated circularly and uniformly; and the child and placenta were naturally expelled. Neither pain, hemorrhage, nor fever followed, and the woman speedily recovered. Still the uterine mouth did not regain its habitual softness, but presented a hard circle; an inevitable effect of the cicatrization of the numerous incisions which had been inflicted. With a view of dilating the orifice, a wax bougie, gradually increased in diameter, was for one week introduced every day, till it could no longer be borne. Eleven months after, the woman again became pregnant, and it was necessary to have recourse a second time to the bistoury, to facilitate the process of parturition; but incisions, less deep than on the former occasion, were now requisite, and the operation was completely successful; as the woman, at the close of a third pregnancy, was delivered without any such assistance.

BOTANY.

Arracacha.—The root of this plant from Santa Fè is expected soon by one of our first naturalists. It grows in a climate of the temperature of England; and from the account given of it in the annals of botany, it promises to equal the potato, although it is not of the family of *solanum*, but probably a *pastinaca*. We cannot have too many good things of this sort; although, it must be owned, we do not yet know the extent of the value of the *solanum tuberosum*.

New Species of Cinchona.—The learned botanist, Lambert, has lately become the possessor of at least forty-five kinds of cinchona, being duplicates to a collection by the emissary Spanish naturalists lately sent over to Madrid from the cinchona forests of America.

Balsam of Peru and of Tolu—Have been ascertained, by M. Lambert, to be from the same species of plant.

We recommend to the grave perusal of a *certain class* of the profession the ensuing article, which is most impressively admonitory, and we hope may be *meditated*, not without the sense of contrition, and the resolution to reformation. Editor.

Dr. Thomson, professor of Chemistry at Glasgow, and Mr. Allan, the author of Mineralogical Tables.—In consequence of certain unfavourable reports which Dr. Thomson had circulated against Mr. Allan of Edinbugrh, an action for damages was brought by the latter against the professor.

This cause, which has excited considerable interest, and has been the subject of much conversation in the scientific circles

for some time past, was appointed for trial January 25th, in consequence of which the court was completely filled on its opening.

The grounds of action alleged by the pursuer, Thomas Allan, esq. banker in Edinburgh, were, that the defender, Dr. Thomas Thomson, professor of chemistry in the University of Glasgow, had expressed himself to persons in that city and its vicinity, in terms which were false and injurious to the pursuer, such as that a friend of his was about to prosecute the pursuer for appropriating to himself certain minerals, and that he had imposed upon mineral dealers. The damages were laid at 5000*l.*

Many applications were made to Mr. Allan, by the friends and the counsel of Dr. Thomas, to accept an apology rather than carry the question into court; Mr. Allan uniformly declared, that being actuated by no vindictive motives to Dr. Thomson, he would not object to receive such an apology as his friend, Mr. Ferguson of Raith, approved of. It was not, however, till the evening before the trial, that the final arrangements were made, when Mr. Ferguson positively refused to listen to any thing but an apology made in open court, and on the express condition that Dr. Thomson defrayed all the expenses incurred. In consequence, when the cause was called, Mr. Cunningham stated to the court, that the pursuer, Mr. Allan, had accepted of an apology from the defender, and that in return Mr. Allan had agreed to give the explanation which Mr. Ferguson had drawn out.

The learned gentleman then read the following apology by Dr. Thomson:

"Having been led, by very false information, to accuse most unjustly Thomas Allan, esq. banker in Edinburgh, on a subject connected with his mineralogical pursuits, I now publicly express my sincere regret for having propagated a most groundless calumny against that gentleman; and do declare, that I now find, that so far from what was reported to me, and repeated by me, having the slightest foundation, Mr. Allan, on the contrary, was the direct means of tracing, and transmitting to the proper owner in London, the minerals which were the subject of the charge. I trust that Mr. Allan and his friends will accept of this apology for the error I have been led into.—Edinburgh, Jan. 21, 1822."

The following explanation was then read by Mr. Cockburn:

"I accept of the apology Dr. Thomson has now made, and take the opportunity which he has now afforded me, of expressing my regret for the harsh language I used towards him, which arose altogether from his repetition of injurious allegations with respect to my conduct and character."

AMERICAN INTELLIGENCE.

We have just received from Drs. Lawrance and Coates the following notice, in anticipation of a report more at length, which they are about to make on the same subject. EDITOR.

ANNUNCIATION.

We have been for some time employed, with the occasional assistance of Dr. Horner and some other gentlemen, in continuing the course of experiments on the subject of absorption, which was begun, last year, by a committee of the Academy of Medicine of this city. We have obtained, by injecting two chemical agents into different parts of the bodies of animals, the singular result of an actual chemical change in them, while contained in living vessels, into which they had been conveyed by a process of absorption. That foreign bodies can be detected in the fluids has often been shown, but it was our object to discover whether they were liable during life to alterations dependent on laws other than those of the vital system. On the 2d of July, 1822, a solution of green sulphate of iron was injected into the cellular substance covering the abdomen of a large cat, and one of prussiate of potass into the cavity itself, so as not to mix. No blue colour was discovered in any part of the animal, but the tests indicated the presence of prussiate of potass in the lymph and urine. Suspecting that the cause of the failure was the circumstance of the difficulty of absorption of the sulphate in the cellular tissue, we repeated the experiment on the next day, with the variation of throwing the sulphate into the abdominal cavity, where we had ascertained that absorption was more easy and rapid, and the prussiate into the cellular substance. In this animal, the fluid of the thoracic duct was found of a deep and strong blue. The coagulating lymph of the blood and nearly the whole of the lungs were also of a strong and bright blue. The urine afforded it distinctly, but in a much less degree. The experiment was afterwards twice repeated, with similar results.

We have ascertained, by several experiments, that the least time in which the prussiate is transferred from the abdominal cavity to the upper part of the thoracic duct, in kittens, varies from two to five minutes. In doing this, we constantly observe, as in our former trials, that none of the salt was discovered in the blood or other fluids, previously to its appearance at the upper extremity of the great lymphatic trunk.

The process of absorption has long been supposed to continue for some time after general death; and this is commonly assigned as a reason for the smaller quantity of blood found in the vessels of the subject, than that supposed to exist during life. We do not know of any direct proof being exhibited, that lymphatic imbibition continues after the circulation of the blood has ceased.

While we constituted together with Dr. Harlan, the committee who tried some experiments during the last summer, we had frequent opportunities of seeing the progress of the fluids contained in the absorbents of dead animals towards their common trunk, but not of ascertaining that articles actually still entered the lymphatics from without. In the *lacteals*, we are informed that it has been seen by Magendie, and we have witnessed the appearances he describes.

On the 12th and 13th of July, we injected prussiate of potass into the abdomen of three kittens after having bled them to death, and in one instance, tied all the large vessels at the heart with a ligature, so as to prevent all circulation. In each instance, absorption of the foreign matter took place distinctly, as evinced by the usual test applied to the fluid of the thoracic duct, but in a considerably longer time than during life. This clearly proves the incorrectness of the opinion advanced by a late physiologist, that the lymphatics are filled exclusively from the arteries.

We have made other experiments, some of which are not without interest; but defer making them public to a future time. J. O'B. LAWRANCE.
B. H. COATES.

A muscle has lately been discovered, by W. E. Horner, M. D. Ass. Prof. Anat. University of Pennsylvania, situated at the internal commissure of the eyelids, on the posterior part of the lachrymal ducts and passing from the flat surface of the os unguis towards the puncta lachrymalia, which has a curious and interesting influence on the position and functions of the puncta and of the eyelids.

The details of this discovery, together with some new remarks on the connexions of the part, may be expected shortly in the next volume of the American Philosophical Transactions.

The Museum of the Anatomical and Surgical Professorships, in the University of Pennsylvania, has been lately transferred to a large and well arranged room, appropriated to the purpose. The additions to it have been very considerable in descriptive and pathological anatomy, and in superb surgical paintings. This collection founded on the private liberality of the widow of the late Dr. Wistar, who at her own expense purchased it from the administrators of her husband's estate, and cherished by the patronage of the trustees, has since received the accession of the beautiful wax preparations of the professor of surgery, and of his magnified drawings in oil colours, and also various contributions from the chair of anatomy. It is now in a state which bids fair in a few years to be fully adequate to all the demands of the departments for which it is intended, and to rival the finest collections of the kind, of Europe.

Great attention is at present directed to a discovery, to which we think Dr. Physick is entitled. He has found, much oftener than might be suspected, that a species of consumption, and in many instances, of a very formidable character, is produced by the irritation from an elongated uvula, and which is relieved by simply cutting off a portion of it Expecting to receive for our next number a communication on the subject, we shall now only further observe, that such is the frequency of the occurrence of this form of the disease, that six cases of it have come under our own observation within the last year, a large proportion of which, we add with pleasure, were cured.

In one of our late numbers, we alluded to the use of the saccharum saturni in several of the affections of the bowels. We have for many years occasionally employed it both in cholera infantum, and dysentery, though never to the same extent, or with as much success, as during the present season. These two diseases have prevailed in this city in a very unusual degree for the last month, which has afforded us ample opportunities of testing the powers of the medicine. Evacuations having been premised, by venesection, and purging, we have found no one remedy more effectually to relieve tormina and tenesmus, to correct the morbid secretions of the intestinal tube, to allay febrile excitement, or to conduce to the comfort, and general improvement in the condition of the patient. These remarks apply especially to dysentery, though in the cases of cholera infantum approaching the same character, we have derived nearly equal advantage from it.

To an adult we give from half a grain to a grain of the lead, with five drops of laudanum, every hour or two, according to the urgency of the symptoms, either in solution or pill.

In some instances, where we were desirous of acting on the surface, we have added a small portion of ipecacuanha, with utility, to the proceding prescription. It meets exceedingly well this particular indication, without at all impairing the specific properties of the lead. In the progress of some protracted cases, it will be necessary about once in the twenty-four hours, to intermit this course of treatment in order to interpose some purgative, to remove any foul accumulations, which may take place in the bowels.

By whom this practice was originated we know not with certainty. It has been vaguely spoken of, and we suspect, as loosely tried, for many years, among us. We have however, reason to believe that the credit is due to Dr. Irvin of Charleston, South Carolina, one of the most intrepid, original, and accomplished physician of our country.

To calm gastric irritability as well in *malignant* as ordinary bilious fevers, and cholera morbus, the saccharum saturni, proves exceedingly useful, for which suggestion, we are undoubtedly indebted to Dr. Irvin. We have also on the authority of Dr. Dewees of this city, used for similar purposes, with the greatest advantage, a strong decoction of coffee, without cream or sugar. It is particularly serviceable in the cholera of children.

The intermittent fever, which so widely prevailed during the last autumn in the neighbourhood of this city and elsewhere, has again recurred, and, for the most part, with great obstinacy. In many instances, it is attended with a singularly irritable stomach so as to preclude the use of bark, and the ordinary remedies. We have found such cases sometimes to yield to emetics repeated for three successive times, on the days of the anticipated paroxysm. There is another practice which has proved very successful under similar circumstances. It is to keep the patient in bed for several days, repeating a dose of opium every six hours, so as not to allow the impression of that article to wear off

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Philadelphia Vaccine Institution.—The subscriber who for several years past, has been entrusted with the vaccination of the poor under an ordinance of the city councils, and to whom numerous applications have consequently been made for furnishing vaccine matter—has heretofore gratuitously distributed it at a considerable expense of time and labour to practitioners of medicine, in this as well as in other states of the Union. Under that ordinance it is his duty to supply the practitioners of the city—but in consequence of the increased demands from every section of the country, and with a view to accommodate with certainty the physicians of the United States, he has determined, by the advice of many of his medical friends, to open an office for the distribution of genuine vaccine virus.

JOSEPH NANCREDE, M. D.

Vaccine Physician, under an ordinance of the Select and Common Councils of of the city of Philadelphia,— *No. 451, South Tenth Street.*

We entirely approve of the above plan for the distribution of vaccine matter, and do believe that the duties it enjoins will be performed by Dr. Nancrede with care and fidelity.

P. S. PHYSICK,
N. CHAPMAN,
THOMAS C. JAMES,
W. GIBSON,
JOHN REDMAN COXE,
JOSEPH HARTSHORNE,
JOHN A. MONGES,
THOMAS T. HEWSON.

N. B. Applications by letter post paid, enclosing three dollars, will at all times receive immediate attention.

The editors of Medical Journals in the United States, are respectfully requested to give the above a few insertions in their respective works.

Philadelphia, May 10, 1822.

BILLS OF MORTALITY.

Statement of Deaths in Boston, from the thirty-first of December, 1821, *to the first of January,* 1822.

DEATHS IN EACH MONTH.	MALES.	FEMALES.	TOTAL.
January,	56	56	112
February,	35	31	66
March,	56	50	106
April,	51	42	93
May,	64	48	112
June,	63	47	110
July,	79	60	139
August,	81	79	160
September,	89	75	164
October,	54	67	121
November,	50	65	115
December,	57	65	122
Totals,	735	685	1420

AGES.			
Under	1	Year	283
From	1	to 2	118
	2	5	116
	5	10	47
	10	20	53
	20	30	136
	30	40	133
	40	50	108
	50	60	79
	60	70	53
	70	80	48
	80	90	22
	90	100	3
	Unknown		151
		Total	1420

The above mentioned DEATHS *were caused by the following* DISEASES *and* CASUALTIES, *viz:*

Abscess,	2	Brought over,	794
Accidental,	1	Gravel,	1
Angina Pectoris,	1	Hemorrhagia,	1
Apoplexy,	7	Hydrocephalus, Internis,	6
Asthma,	1		
Burns,	13	Hydrothorax,	2
Cancer,	3	Hooping Cough,	26
Casualty,	17	Infantile Diseases,	153
Cholera Infantum,	6	Insanity,	4
Morbus,	9	Intemperance,	30
Chronic Diarrhœa,	9	Jaundice,	3
Dysentery,	4	Lumbar Abscess,	1
Consumption,	192	Marasmus,	1
Cynanche Trachealis,	3	Measles,	149
Croup,	11	Mortification,	8
Diseases of the Heart,	4	Murdered,	1
Diseases unknown,	243	Old Age,	31
Diseased Scapula,	1	Paralysis,	22
Debility,	8	Phthisis,	23
Delirium Tremens,	5	Phrenitis,	13
Dropsy,	32	Quinsy,	5
Drowned,	19	Scald,	1
Drunkenness,	1	Scrophula,	5
Dysentery,	60	Scirrhus, Liver,	5
Dyspepsia,	3	Spleen,	1
Enteritis,	10	Uterus,	1
Epilepsy,	1	Syphilis,	1
Fever,	8	Spina Bifida,	1
Bilious,	10	Spasms,	3
Inflammatory,	2	Still-born,	116
Nervous,	1	Sudden,	6
Puerperal,	7	Suicide,	2
Pulmonic,	31	Suffocation,	1
Rheumatic,	6	Tuberculated Phthisis,	1
Scarlatina,	1	Ulcerated Stricture of Intestine,	1
Typhus,	42		
Fits,	19	White Swelling,	1
Carried over,	794	Total	1420

N. B. There were eleven deaths in the hospital on Rainsford Island during the above period, viz. nine of yellow fever, and two of chronic diarrhœa. The number of inhabitants, in the town, by the last Census, were 43,893. Boston lies in 42° 23′ 15 N. lat. and 70° 32′ 42 W. long.

Published by Order of the Board of Health.

BENJAMIN WHITMAN, jr. Secretary.

Statement of Deaths in the City and County of New York, from the first of January, 1821, to the first of January, 1822.

DEATHS IN EACH MONTH.	MALES.	FEMALES.	TOTAL.
January,	145	142	287
February,	119	130	249
March,	155	149	304
April,	129	142	271
May,	146	111	257
June,	108	115	223
July,	195	136	331
August,	225	231	456
September.	184	176	360
October,	172	151	323
November,	124	98	222
December,	164	95	259
Totals,	1866	1676	3542

AGES.		
Under 1 Year		825
From 1 to	2	369
2	5	261
5	10	122
10	20	158
20	30	445
30	40	446
40	50	346
50	60	243
60	70	175
70	80	79
80	90	57
90	100	15
100	110	1
	Total,	3542

The above mentioned DEATHS *were caused by the following* DISEASES *and* CASUALTIES, *viz:*—

Abscess,	10	Brought over,	1126
Aneurism,	1	Drinking Cold Water,	2
Apoplexy,	45	Dropsy,	108
Asphyxia,	1	in the Chest,	27
Asthma,	12	in the Head,	152
Burned or Scalded,	17	Drowned,	36
Carbuncle,	1	Dysentery,	142
Cancer,	14	Dyspepsia,	5
Casualty,	28	Epilepsy,	10
Catarrh,	2	Erysipelas,	2
Chicken Pox,	2	Fever,	121
Child-bed,	19	Bilious,	21
Cholera Morbus,	14	Remittent,	51
Colic,	12	Inflammatory,	3
Compression of the Brain,	3	Intermittent,	7
		Malignant,	3
Consumption,	715	Puerperal,	4
Convulsions,	160	Putrid,	1
Contusion,	6	Scarlet,	3
Cramp in Stomach,	3	Typhus,	127
Diabetes,	2	Flux, Infantile,	114
Diarrhœa,	49	Fracture,	4
Carried over,	1126	Carried over,	2069

Brought over,	2069	Brought over,	2887
Gout,	2	Peripneumony,	20
Hemorrhage,	14	Pleurisy,	15
Hemoptysis,	4	Pneumonia Typhodes,	5
Herpes,	1	Quinsy,	15
Hives,	125	Rheumatism,	7
Infanticide,	5	Rickets,	2
Inflammation of the Bladder,	2	Rupture,	5
		St. Anthony's Fire,	1
of the bowels,	49	Scirrhus of the Liver,	2
of the Brain,	28	Scrofula, or King's Evil,	12
of the Chest,	115	Sore Throat,	10
of the Liver,	27	Spasms,	4
of the Stomach,	11	Spina Bifida,	2
Insanity,	6	Sprue,	25
Intemperance,	64	Still-born,	174
Jaundice,	16	Sudden Death,	18
Locked Jaw,	7	Suicide,	16
Lumbar Abscess,	3	Syphilis,	9
Manslaughter,	1	Tabes Mesenterica,	96
Marasmus,	49	Teething,	30
Measles,	109	Tumour,	1
Menorrhagia,	1	Vomiting Blood,	1
Mortification,	16	Ulcer,	13
Nervous Disease,	4	Unknown,	62
Old Age,	122	Whooping Cough,	92
Palsy,	37	Worms,	18
Carried over,	2887	Total,	3542

OF THE ABOVE DEATHS, THERE WERE :—

Men,	974	
Boys,	892	
Total Males,		1866
Women,	835	
Girls,	841	
Total Females,		1676
Total Number,		3542

REMARKS.

The City Inspector respectfully reports to the Board a statement of the deaths in the city and county of New York, for the year 1821, amounting to *three thousand five hundred and forty-two*, being *twenty-seven* more than reported the preceding year.

The consumptive cases, as usual, form an important item in the list: *seven hundred and fifteen* are recorded as victims to

this fatal complaint, being one fifth of the whole number of deaths. The males were *three hundred and sixty-two;* the females, *three hundred and fifty-three,* of which there were forty-seven coloured males, and fifty-eight females.

The deaths of coloured persons for the year, were *five hundred and fifty.*

The number of fever cases were *three hundred and forty-one,* being less than in the preceding year. And although the *malignant fever* prevailed at and in the neighbourhood of the quarantine ground, it was scarcely felt here, as there were but *three* well attested cases reported, and these were supposed to have been brought from that place.

The diseases incident to the summer months were less fatal than in the former year; and the disappearance of *small pox* in our city since July, 1818, which had been so long a scourge to every part of the world, has proved the value of the *vaccinating system,* as one of the happiest discoveries in modern times.

GEORGE CUMMING, City Inspector.

City Inspector's Office, 21st January, 1822.

Statement of Deaths in the City and Liberties of Philadelphia, from the first of January, 1821, *to the first of January,* 1822.

DEATHS IN EACH MONTH.	ADULTS.	CHILDREN.	TOTAL.
January,	114	87	201
February,	114	101	215
March,	134	105	239
April,	126	98	224
May,	143	102	245
June,	151	106	257
July,	162	95	264
August,	200	252	452
September,	146	121	267
October,	175	121	296
November,	164	126	290
December,	129	93	222
Totals,	1765	1407	3172

AGES.			
Under	1	Year	844
From	1	to 2	215
	2	5	193
	5	10	102
	10	15	50
	15	20	88
	20	30	405
	30	40	396
	40	50	324
	50	60	221
	60	70	154
	70	80	97
	80	90	61
	90	100	21
	100	110	1
		Total,	3172

The above mentioned DEATHS *were caused by the following* DISEASES *and* CASUALTIES, *viz:*—

Apoplexy,	49	Brought over,	1715
Atrophy,	33	Effusion,	2
Asthma,	6	Fever,	55
Angina Pectoris,	16	Typhus,	106
Aneurism,	2	Bilious,	118
Aphthæ,	1	Remittent,	93
Abscess,	23	Intermittent,	3
Amenorrhœa,	2	Inflammatory,	7
Burns,	20	Scarlet,	13
Consumption of the Lungs,	438	Nervous,	11
		Hectic,	4
Convulsions,	153	Puerperal,	9
Cholera Morbus,	191	Fractures,	2
Cancer,	14	Found Dead,	7
Catarrh,	25	Gout,	6
Concussion,	3	Gangrene and Mortification,	14
Contusion,	4	Hydrophobia,	1
Casualties,	13	Hives,	63
Child-bed,	6	Hemorrhage,	16
Colic,	11	Hernia,	4
Cachexy,	3	Hooping Cough,	36
Coma,	1	Inflammation of the Lungs,	110
Caries,	2	Liver,	46
Dropsy,	73	Brain,	29
in the Head,	99	Bowels,	53
of the Breast,	22	Bladder,	2
Debility,	173	Breast,	13
Diarrhœa,	65	Stomach,	8
Dysentery,	124	Kidneys,	4
Decay,	32	Peritoneum,	8
Drunkenness,	22	Heart,	2
Disease of the Heart,	3	Pericardium,	1
Death by Violence,	3	Uterus,	2
Drinking Cold Water,	5	Insanity,	28
Disease of Hip Joint,	4	Inanition,	1
Death by the Cold,	1	Jaundice,	3
Drowned,	42	Locked Jaw,	10
Dyspepsia,	6	Lethargy,	2
Epilepsy,	14	Leprosy,	1
Erysipelas,	9	Laudanum to excess,	5
Epistaxis,	1		
Eruptions,	1		
Carried over,	1715	Carried over,	2513

Brought over,	2513	Brought over,	2725
Mania à Potu,	23	Suicide,	7
Old age,	42	Spasm,	4
Palsy,	40	Stone,	4
Pleurisy,	11	Strangury,	1
Peripneumonia Biliosa,	1	Still-born,	211
Phlegmasia Dolens,	4	Teething,	14
Rheumatism,	7	Tumours,	4
Suffocation,	5	Tympanitis,	1
Scrofula,	17	Ulcers,	7
Spina Bifida,	3	Urticaria,	1
Syphilis,	3	Worms,	10
Sore Throat,	25	Wounds,	2
Sudden,	30	Unknown,	84
Carried over,	2725	Total,	3172

OF THE ABOVE DEATHS, THERE WERE:—

Males of 20 years and upwards,	937	
Ditto under 20 years,	721	
		1658
Females of 20 years and upwards,	636	
Ditto under 20 years,	588	
		1224
Children, principally under one year, whose sex is unknown,		290
Total,		3172

Of the foregoing deaths, 368 died in the Alms House, and 686 were people of colour.

Agreeably to the returns received at the Health Office from *ninety-nine* practitioners of midwifery, there were born, in the city and Liberties, from the 1st of January, to the 31st of December, 1821,

Male children,	2630
Female ditto,	2417
Making the total number of BIRTHS,	5047
The whole number of DEATHS,	3172
Difference between *births* and *deaths*,	1875

By order of the Board of Health.

JOSEPH PRYOR, *Clerk.*

HEALTH OFFICE, January 30, 1822.

Statement of Deaths in the city of Baltimore, from the first of January, 1821, to the first of January, 1822.

DEATHS IN EACH MONTH.	MALES.	FEMALES.	TOTAL.
January,	72	43	115
February,	72	73	145
March,	35	39	74
April,	65	51	116
May,	56	40	96
June,	58	33	91
July,	120	87	207
August,	161	128	289
September,	170	121	291
October,	155	112	267
November,	93	79	172
December,	87	65	152
Totals,	1144	871	2015

AGES.		
Under	1 Year	428
From	1 to 2	134
	2 5	116
	5 10	87
	10 20	170
	20 30	247
	30 40	283
	40 50	228
	50 60	134
	60 70	83
	70 80	48
	80 90	37
	90 100	15
	100 110	5
Total,		2015

The above mentioned DEATHS *were caused by the following* DISEASES *and* CASUALTIES *viz:*—

Abscess of the Lungs,	2	Brought over,	834
Apoplexy,	24	Drinking Cold Water,	1
Asthma,	6	Drowned,	30
Ascites,	1	Dysentery,	41
Burn,	10	Dropsy,	35
Brain, Organic Disease of,	1	in the Head,	31
Cancer,	6	in the Breast,	4
Casualty,	21	Fever, Bilious,	156
Child-bed,	21	Remittent,	3
Cholera Morbus,	12	Bilious Remittent,	4
Infantum,	197	Intermittent,	18
Colic,	7	Typhus,	33
Bilious,	8	Nervous,	5
Cramp,	11	Scarlet,	3
Consumption,	336	Inflammatory,	4
Convulsions,	84	Malignant,	173
Cramp in Stomach,	1	Fistula in Ano,	1
Croup,	48	Gangrene,	1
Cynanche Maligna,	1	Gravel,	2
Carbuncle,	1	Hemorrhage,	3
Chicken Pox,	1	Hemorrhage Uterine,	3
Decay,	24	Hives,	1
Diarrhœa,	10	Hemoptysis,	1
Chronic,	1	Intemperance,	47
Carried over,	834	Carried over,	1434

Brought over,	1434	Brought over,	1685
Insanity,	5	Rheumatism in the Head,	1
Inflammation,	3	Rupture,	1
of the Stomach,	4	Scirrhus,	1
of the Brain,	22	Scald,	1
of the Bowels,	2	Scrofula,	5
of the Liver.	1	Small Pox,	21
Liver Complaint,	20	Still-born,	105
Locked Jaw,	3	Sudden Death,	15
Measles,	2	Suicide,	2
Marasmus,	15	Syphilis,	6
Mortification,	13	Tetanus,	1
Murdered,	6	Teething,	9
Mumps,	1	Tumour,	2
Old Age,	93	in Abdomen,	1
Palsy,	18	Thrush,	2
Pneumonia,	1	Unknown, Adults,	15
Typhoid,	2	Infants,	88
Pleurisy,	30	Ulcer,	4
Pemphigus,	1	Whooping Cough,	28
Quinsy,	4	Worms,	21
Rheumatism,	5	Wounds,	1
Carried over,	1685	Total,	2015

Of the deaths above numerated 423 were people of colour.

By order of the Board of Health.

WILLIAM D. HARRIS, Secretary.

Statement of Deaths in the city of Charleston, (South Carolina,) from the first of January, 1821, to the first of January, 1822.

DEATHS IN EACH MONTH.	MALES.	FEMALES.	TOTAL.
January,	36	27	63
February,	39	30	69
March,	30	26	56
April,	31	20	51
May,	49	32	81
June,	36	64	100
July,	75	40	115
August,	55	48	103
September,	43	43	86
October,	44	44	88
November,	29	22	51
December,	37	21	58
Totals,	504	417	921

AGES.		
Under	3 Years	269
From 3	to 10	65
10	20	69
20	30	125
30	40	128
40	50	85
50	60	63
60	70	49
70	80	45
80	90	16
90	100	5
100	110	1
110	120	1
	Total,	921

The above mentioned DEATHS *were caused by the following* DISEASES *and* CASUALTIES, *viz*:—

Abscess,	4	Brought over,	675
Accident,	20	Gravel,	4
Apoplexy,	11	Hemorrhage,	3
Asthma,	4	Hooping Cough,	6
Cancer,	3	Inflammation of the Brain,	2
Chicken Pox,	2		
Child-bed,	7	Inflammation of the Lungs,	4
Cholera Morbus,	1		
Colic,	6	Influenza,	1
Constipation,	1	Insanity,	7
Consumption,	164	Intemperance,	9
Convulsions,	48	Jaundice,	1
Cramp,	1	Liver Complaint,	7
Croup,	9	Locked Jaw,	17
Debility,	57	Measles,	14
Diarrhœa,	96	Mortification,	3
Dropsy,	83	Old Age,	43
Drowned,	11	Palsy,	11
Dysentery,	12	Pleurisy,	6
Enteritis,	1	Rheumatism,	13
Erysipelas,	2	Rupture,	1
Fever, Bilious,	33	Scrofula,	5
Catarrhal,	33	Sore Throat,	38
Country,	21	Spasms,	18
Intermittent,	1	Sudden Death,	8
Nervous,	11	Suicide,	2
Remittent,	2	Teething,	20
Typhus,	4	Thrush,	2
Worm,	26	Violence,	1
Fistula,	1		
Carried over,	675	Total,	921

OF THE ABOVE THERE WERE:—

White Males,	258	
White Females,	152	
Total Whites,		410
Black Males,	246	
Black Females,	265	
Total Blacks,		511
Total Deaths,		921

*** The cases of consumptions were generally strangers, who came to Charleston for the benefit of their health.

By order of the Board.

JAMES A. MILLER, Clerk.

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